1368

UNDERGROUND USES OF NUCLEAR ENERGY

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HEARINGS

BEFORE THE

SUBCOMMITTEE ON AIR AND WATER POLLUTION

OF THE

COMMITTEE ON PUBLIC WORKS UNITED STATES SENATE

NINETY-FIRST CONGRESS

FIRST SESSION

ON

S. 3042

A BILL TO PROVIDE FOR A STUDY AND EVALUATION OF THE AIR AND WATER POLLUTION AND OTHER ENVIRONMENTAL EFFECTS OF UNDERGROUND USES OF NUCLEAR ENERGY FOR EXCAVATION, AND OTHER PURPOSES

NOVEMBER 18, 19, AND 20, 1969

Printed for the use of the Committee on Public Works

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UNDERGROUND USES OF NUCLEAR ENERGY

TUESDAY, NOVEMBER 18, 1969

U.S. SENATE,
SUBCOMMITTEE ON AIR AND WATER POLLUTION,
OF THE COMMITTEE ON PUBLIC WORKS,
Washington, D.C.

The Subcommittee on Air and Water Pollution met at 9:40 a.m., pursuant to call, in room 4200, New Senate Office Building, Senator Edmund S. Muskie (chairman of the subcommittee) presiding.

Present: Senators Muskie, Randolph, Gravel, and Baker.

Also present: M. Barry Meyer, counsel; Leon G. Billings and Richard D. Grundy, professional staff members, and Walter Planet, Department of Commerce Fellow.

OPENING STATEMENT OF HON. EDMUND S. MUSKIE, CHAIRMAN OF THE SUBCOMMITTEE ON AIR AND WATER POLLUTION OF THE COMMITTEE ON PUBLIC WORKS

Senator MUSKIE. The committee will be in order.

Today, the Subcommittee on Air and Water Pollution opens its hearings on S. 3042 which would establish a commission to examine the potential environmental effects of underground uses of nuclear energy for excavation and certain other purposes.

This committee has long been concerned with the impact of new technology on the quality of the environment.

Evidence of this concern can be found in past hearings on various air and water pollution problems associated with waste heat discharges from nuclear powerplants.

With the tremendous development in the field of nuclear energy in the past decade, and particularly with the increasing possibility of its extensive use for underground excavation and exploration, the potential environmental problems associated with these uses must be explored now.

These hearings will focus on the present level of understanding of the environmental implications of the underground use of nuclear energy and the need for additional independent review. Many questions need to be asked.

We have been too dependent in the past on the predictive judgments of a few experts, instead of seeking evaluations by independent observers.

In these hearings we will hear from witnesses with experience in the problems of radioactive contamination of the environment.

I anticipate that their testimony will provide information on which to determine the necessity of establishing either the commission suggested in Senator Gravel's legislation or some other means of independent review of the potential environmental effects of these applications of nuclear energy.

I would like to emphasize at this point, however, that the subcommittee and the full committee are sensitive to the fact that the principal jurisdiction over the development of nuclear energy rests in the Joint Committee on Atomic Energy.

It is not the intent of this committee to trespass upon that jurisdiction and we will undertake to maintain our sensitivity.

So there are many applications of nuclear energy, both military and nonmilitary, which we will skirt and which we will seek not to invade by these hearings.

I think it might be helpful at this point to put in the record some preliminary discussion of these issues prior to the testimony of the witnesses so that the reader of the record may benefit from them.

These include a copy of S. 3042. Following this, we have a letter from the Bureau of the Budget with their comment on S. 3042. Agency reports are also included herein plus a communication from Representative Chet Holifield, chairman of the Joint Committee on Atomic Energy. Also, an excellent briefing paper prepared by the staff with several attachments, the first of which lists the potential applications of underground testing with reference to canal construction, mining, hydropower projects, water resource projects, harbor construction, and general transportation construction.

Attachment B gives the legislative history. Attachment C gives the scientific and engineering properties of nuclear explosion.

Attachment D gives the potential schedule of underground explosions as we look ahead.

Attachment E gives our experience to date.

Finally, there is a table which lists the experimental tests and the underground uses of nuclear explosions under the Plowshare program.

I think all of this preliminary information at the outset of the record would be very helpful.

(The materials referred to follow:)

[S. 3042, 91st Cong., first sess.]

A BILL To provide for a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation, and other purposes

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That—

FINDINGS

The Congress finds that radioactive materials can migrate through the land, air, and water environment and can be concentrated in biological organisms which serve as food supply, and that the ecological implications of the use of underground nuclear energy have not been adequately explored.

ESTABLISHMENT OF THE COMMISSION

SEC. 2. (a) There is hereby established a Commission which shall be composed of fifteen members to be appointed by the President. The majority of such Commission shall be selected on the basis of recognized expertise in the fields

of air, water, and land pollution, and the remainder of the members shall be selected on the basis of special knowledge and expertise in disciplines related to the human and physical environment.

(b) No member of the Commission at the time of his appointment shall be in the employ of the Government, or under contract with the Government, or otherwise engaged in research or consultation for the Government as the employee of a private business organization under contract with the Government: *Provided, however*, That this subsection shall not operate as a bar to the appointment of a person, not a Government employee, whose work under such contract is not directly related to the functions of the Commission as set forth below.

(c) Any vacancy in the Commission shall not affect its powers.

(d) The President shall designate one of the members to serve as Chairman and one to serve as Vice Chairman of the Commission.

(e) Eight members of the Commission shall constitute a quorum.

DUTIES OF THE COMMISSION

SEC. 3. (a) The Commission shall undertake a comprehensive investigation and study of the effect on public health and welfare of the use of nuclear energy for excavation and other underground applications. Such study shall include among other things air, water and land pollution resulting from radioactive releases associated with such use. The Commission shall evaluate the following environmental risks attendant upon the underground use of nuclear energy:

(1) the effect on the public health and welfare;

(2) the effect on terrestrial, marine, and fresh water ecosystems, including the generation of earthquakes and other seismic activity and other geophysical phenomena; and

(3) the transport of radioactivity through the environment.

In addition, the Commission shall evaluate the risks attendant upon the use of nuclear energy to remove or lower natural biological barriers which may cause the introduction of non-indigenous species into preexisting ecosystems.

(b) The Commission shall transmit to the President and to the Congress such interim reports of its findings as it deems necessary or advisable.

(c)(1) The Commission shall transmit to the President and to any authorized board, council, or office of environmental quality and to the Congress no later than one year after the first meeting of the Commission a final report containing a detailed statement of the findings and conclusions of the Commission, together with its recommendations, including such recommendations for legislation and administrative action as it deems advisable regarding the use of nuclear energy for such purposes.

(2) No action shall be taken on any report required by any other provision of law relevant to the use of nuclear energy for excavation purposes until the Commission has transmitted the final report required by this Act.

POWERS OF THE COMMISSION

SEC. 4. (a) The Commission or, on the authorization of the Commission, any subcommittee or members thereof, may, for the purpose of carrying out the provisions of this Act, hold such hearings, take such testimony, and sit and act at such times and places as the Commission deems advisable. Any member authorized by the Commission may administer oaths or affirmations to witnesses appearing before the Commission or any subcommittee or members thereof.

(b) Each department, agency, and instrumentality of the executive branch of the Government, including independent agencies, is authorized and directed to furnish to the Commission, upon request made by the Chairman or Vice Chairman, such information as the Commission deems necessary to carry out its functions under this Act.

(c) Subject to such rules and regulations as may be adopted by the Commission, the Chairman shall have the power to—

(1) appoint and fix the compensation of an executive director, and such additional staff personnel as he deems necessary, without regard to the provisions of title 5, United States Code, governing appointments in the competitive service, and without regard to the provisions of chapter 51 and subchapter III of chapter 53 of such title relating to classification and General Schedule pay rates, but at rates not in excess of the maximum rate for GS-18 of the General Schedule under section 5332 of such title, and

(2) procure temporary and intermittent services to the same extent as is authorized by section 3109 of title 5, United States Code, but at rates not to exceed $50 a day for the individuals.

(d) The Commission is authorized to enter into contracts with Federal or State agencies, private firms, institutions, and individuals for the conduct of research or surveys, the preparation of reports, and other activities necessary to the discharge of its duties.

COMPENSATION OF MEMBERS

SEC. 5. Members of the Commission shall receive compensation at the rate of $100 per day for each day they are engaged in the performance of their duties as members of the Commission and shall be entitled to reimbursement for travel, subsistence, and other necessary expenses incurred by them in the performance of their duties as members of the Commission.

APPROPRIATIONS AUTHORIZED

SEC. 6. There are hereby authorized to be appropriated out of any money in the Treasury not otherwise appropriated, such sums as may be necessary to carry out the provisions of this Act.

TERMINATION

SEC. 7. On the ninetieth day after the date of submission of its final report to the President and the Congress, the Commission shall cease to exist.

AGENCY COMMENTS ON S. 3042

(Bureau of the Budget:)

EXECUTIVE OFFICE OF THE PRESIDENT,
BUREAU OF THE BUDGET,
Washington, D.C., December 4, 1969.

Hon. JENNINGS RANDOLPH,
Chairman, Committee on Public Works,
U.S. Senate, Washington, D.C.

DEAR SENATOR RANDOLPH: This is in reply to your letter of October 20, 1969, in which you requested our comments on S. 3042, a bill "To provide for a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation, and other purposes."

The Atomic Energy Commission, the Office of Science and Technology, and other Executive branch agencies also are reporting to you on this bill. For the reasons set forth in those reports, we do not recommend enactment of S. 3042.

Sincerely yours,

WILFRED H. ROMMEL,
Assistant Director for Legislative Reference.

(Department of State:)

DEPARTMENT OF STATE,
Washington, D.C.

Hon. JENNINGS RANDOLPH,
Chairman, Committee on Public Works,
U.S. Senate.

DEAR MR. CHAIRMAN: I refer to your letter of October 20, 1969, in which you requested the Department's comments on S. 3042, regarding the environmental effects of underground uses of nuclear energy for excavation.

I believe that I should express the Department's feeling that present arrangements for consideration of the environmental effects of nuclear explosions appear to be satisfactory. The opinions of qualified experts within the various agencies of our Government, as well as those from private U.S. organizations, are obtained and considered when decisions are made regarding the environmental and other effects of underground nuclear explosion experiments. It might be questioned whether it is necessary to duplicate this work by the creation of an

additional organization to conduct similar studies, particularly in view of the time that would be lost in proceeding with the determination of the technological and economic feasibility of nuclear excavation—including the radiation safety aspect of the program.

As your Committee is aware, Article V of the Non-Proliferation Treaty provides that the potential benefits from the peaceful applications of nuclear explosions will be made available to non-nuclear-weapon states party to the Treaty. It is hoped that steady, careful progress can be maintained toward the determination of these benefits, so that foreign governments interested in these programs do not erroneously draw the conclusion that we have discontinued our work toward this goal. Consideration of environmental effect would naturally have to be included in any such studies.

There are several other features of the bill on which the Department offers its comments. We note that the last paragraph of Sec. 3(a) of the bill (page 3, lines 19–22) provides that the "Commission shall evaluate the risks attendant upon the use of nuclear energy to remove or lower natural biological barriers which may cause the introduction of non-indigenous species into pre-existing ecosystems." Inclusion of this provision in this bill would seem to imply that the risks involved are related solely to the use of *nuclear* excavation. However, as we understand it, the problems associated with the mixture of species could result from the removal or lowering of natural biological barriers, regardless of the means employed. That is, the consequences would presumably be the same whether the barriers were removed by nuclear explosions, conventional explosives, or other conventional means.

We also note that Section 2(a) of the bill (page 2, lines 10–15) would prohibit membership on the Commission by Government employees, consultants, or contractors. While this is not directly a matter of concern to this Department, it might be questioned whether or not this provision could prevent the participation of highly qualified experts whose preeminence in the field has already been recognized by the Government agencies concerned and who have consequently been engaged under contract as consultants to the agencies which are concerned with the environmental effects of their programs. It would seem to be in the national interest that if a Commission, such as the one proposed in the bill under discussion, is established it should have recourse to the best available advice in this important field. It would therefore seem unfortunate if those leading experts would be barred from consideration for appointment to the Commission which this bill proposes.

Section 3(c)(2) of the bill (page 4, lines 10–13) might appear to be unnecessarily restrictive, if it were to be interpreted, for example, as prohibiting further studies or further consideration of any report which might have a bearing on US policy toward construction of an Atlantic-Pacific Canal or any other vital US projects in this field.

The Bureau of the Budget advises that from the standpoint of the Administration's program there is no objection to the submission of this report.

Sincerely yours,

H. G. TORBERT, Jr.,
Acting Assistant Secretary for Congressional Relations.

(Atlantic-Pacific Interoceanic Canal Study Commission:)

ATLANTIC-PACIFIC INTEROCEANIC CANAL STUDY COMMISSION,
Washington, D.C., November 19, 1969.

Hon. JENNINGS RANDOLPH,
Chairman, Committee on Public Works,
U.S. Senate, Washington, D.C.

DEAR SENATOR RANDOLPH: This letter is to provide you the views of this Commission on certain aspects of S. 3042, a bill "To provide for a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation, and other purposes."

This bill would create a Commission of fifteen scientific experts appointed by the President to evaluate the environmental risks attendant upon the underground use of nuclear energy and to report its findings to the President and the Congress within a year.

The Canal Study Commission is in full accord with the objectives sought by
S. 3042. The safety aspects of nuclear canal excavation have required a great
portion of our investigative effort and our funds. A controlling determinant of
the feasibility of nuclear canal excavation is assurance that it would not create
significant threats to the environment.

When our five year investigation is completed in 1970 we will have an extensive
evaluation of the potential effects of the radioactivity, ground shock, and air blast
that could be created by nuclear canal excavation. In addition, we will have an
evaluation of the potential ecological effects of linking the oceans at sea-level to
the extent possible in the time available. We were advised early in our investi-
gation that this latter evaluation could only be judgemental at best, and that it
should be based upon observations and experiments conducted over a much
longer period than possible during our five-year study. For this reason we have
contracted with the National Academy of Sciences to develop the study program
that should be undertaken if a decision is made to construct a sea-level canal.

In your Committee's consideration of the potential ecological impacts of a sea-
level canal it should be kept in mind that the present canal has permitted rather
extensive transports of marine life between the oceans for more than a half cen-
tury with no discernible ill effects. This transport takes place by three distinct
means. First, swimming and drifting biota that thrive in both salt and fresh
water readily pass through the locks and inevitably make their way across Gatun
and Miraflores Lakes to the opposite oceans. Some have been specifically identi-
fied as having followed this path. Second, barnacles and similar clinging or-
ganisms pass in both directions every day on the hulls of ships. Third, and per-
haps most important to the question of the biological impact of linking the oceans,
is the daily transfer of fairly large amounts of salt water in ships' ballast tanks.
This has gone on for more than a half century. Lightly loaded or empty ships
approaching the canal are frequently required to take on ballast water before
entering the locks. This is to deepen their drafts to make them easier to handle
while in restricted canal channels. As a usual practice on leaving the canal a few
hours later at the opposite ocean, this ballast water is discharged to lighten the
ships to save fuel on the remainder of the trip. Thus, all the small swimming and
drifting marine life that would be found in these thousands of samples of sea
water taken year in and year out since 1914 have made the trip across the
isthmus in salt water in both directions. While a sea-level, salt-water channel
between the oceans would vastly augment the movements of marine creatures
between the oceans, the new avenue would appear to offer previously denied
passage for only that portion of ocean life that could not transit by one or more
of the three existing means. New exposures through a sea-level canal would be
limited to the larger swimming and drifting biota. Thus the area of danger of
harmful biological changes when the oceans are joined is much less broad than
it first appears.

Our studies thus far, being conducted by the Battelle Memorial Institute, have
identified variants of all the important local species of food fishes and crustaceans
on both sides of the Isthmus. This has led to a tentative conclusion by Battelle
that mixing them would not hazard marine food sources. So far the study has
found no grounds for serious concern over the prospect of joining the oceans.
However, it is yet to be completed. We will report its findings in our final report
next year.

In view of the great effort already being devoted to the objectives of S. 3042
by existing agencies and the additional activities that are planned, the proposed
mission would overlap and duplicate the work of others. The Canal Study Com-
mission does not believe that the current and planned explorations of the eco-
logical implications by existing agencies are inadequate. In particular, we do
not believe that it will be possible in one year to add materially to this Com-
mission's on-going evaluation of the potential ecological effects of linking the
oceans. To this extent, the Canal Study Commission opposes creation of a new
temporary commission for the purpose described in S. 3042.

The Bureau of the Budget has advised that there is no objection to the presen-
tation of these comments from the standpoint of the Administration's program.

Respectfully,

ROBERT B. ANDERSON, *Chairman*.

1126524

(Atomic Energy Commission:)

U.S. ATOMIC ENERGY COMMISSION,
Washington, D.C., November 21, 1969.

Hon. JENNINGS RANDOLPH,
Chairman, Committee on Public Works,
U.S. Senate.

DEAR SENATOR RANDOLPH: This letter is in response to your request for the
comments of the Atomic Energy Commission on S. 3042, a bill "To provide for
a study and evaluation of the air and water pollution and other environmental
effects of underground uses of nuclear energy for excavation, and other pur-
poses."

This bill would create a Commission of fifteen scientific experts appointed by
the President to evaluate the environmental risks attendant upon the under-
ground use of nuclear energy and to report its findings to the President and the
Congress within a year.

The Atomic Energy Commission supports the objective sought by S. 3042—
that the environmental risks related to underground nuclear events should be
subject to comprehensive and objective evaluation by experts in all pertinent
environmental disciplines. We believe that this objective has been and is being
achieved in the Commission's nuclear testing program both with respect to weap-
ons and with respect to peaceful applications. Therefore, we disagree with the
basic rationale for the bill as expressed in the "Findings"—that the "ecological
implications of the use of underground nuclear energy have not been adequately
explored."

No nuclear detonation is ever undertaken by the AEC without adequate pro-
vision for all safety aspects, including potential environmental hazards. This is
achieved through established regular procedures, including long-range studies
of environmental effects as well as detailed operational planning and execu-
tion for each nuclear event. We are confident that these procedures produce ob-
jectivity since the participants include experts with outstanding professional
reputations from universities and industry, as well as from Governmental agen-
cies other than AEC, such as USGS, USC&GS, PHS and ESSA. For example,
members of AEC's Panel of Safety Consultants include experts in ecology, geol-
ogy, hydrology, soil and rock mechanics, structural engineering, seismicity and
tsunamis. The National Academy of Sciences has provided the AEC with some
nominees for membership on this panel and, occasionally, other sources of can-
didates are used. A list of such non-AEC experts is attached as Enclosure A.

Section 2(b) of the bill precludes the appointment to the proposed Commis-
sion of persons who are associated with related work of the AEC as contractors
or contractor employees, or who are employed by the Government. We believe
that it would be very difficult to find enough people with the required program
knowledge who are not associated with some phase of such AEC work. We be-
lieve that objectivity can be expected from the professional scientists and en-
gineers who serve as independent safety consultants and environmental effects
advisors to the AEC under existing arrangements. It should be noted that their
responsibilities relate solely to safety responsibilities and they have no other
AEC program responsibility.

Since the AEC utilizes the best available expertise in studying the safety of
underground nuclear tests, it appears unlikely that reexamination of existing
data by yet another group will provide any new insights. In fact, we have found
only a limited number of available experts in many of the fields involved in the
environmental effects of nuclear detonations. There should, therefore, be a real
concern that the creation of a new Commission will result in findings and recom-
mendations being made by less knowledgeable individuals. The alternative would
be to deplete the number of competent experts presently advising the AEC and
its field test organization.

The development of several of the disciplines related to the physical environ-
ment, such as seismology and oceanography, in order to improve on the ability
to predict earthquakes and sea disturbances and the interrelations between
various types of ground shocks necessitates a pootracted scientific approach. New
data can be developed only by an evolutionary series of progressive experiments,
each building on the knowledge already acquired. For this reason, it is difficult
to see how the proposed Commission could in the one year of its authorized exist-
ence throw much further light on the subject.

In short, the proposed duties of the fifteen man Commission are now being effectively carried out generally by the AEC and other Government agencies. It is our understanding also that the Atlantic-Pacific Interoceanic Canal Study Commission is specifically investigating the last area enumerated in Section 3(a)—"the evaluation of risks attendant upon the use of nuclear energy to remove or lower natural biological barriers which may cause the introduction of non-indigenous species into preexisting ecosystems."

While we believe that we can rely on the adequacy and objectivity of our safety reviews, there is still a further assurance in the requirement that Presidential approval of all nuclear detonations be obtained. If incident to such approval, further scientific study and review should be desired, the President has readily available his Science Adviser and other technical resources within the various Executive agencies. In fact, the Under Secretaries Committee of the National Security Council including representatives from State, Defense, ACDA, NSC and the Science Adviser, together with technical assistance as necessary from USGS, USC&GS, PHS and ESSA, reviews the entire underground test program annually and, in addition, reviews the program again on a quarterly basis and provides individual review to events of a special nature. The Committee's reviews are made from the standpoint of all factors, e.g., political implications, national security requirements, conformity with international agreements, public safety, etc., and in the light of all these considerations, the Committee makes its recommendations for the conduct of such detonations to the President.

Congress, through the Joint Committee on Atomic Energy, also oversees the conduct of the nuclear test program and is apprised of each planned nuclear detonation well before actual detonation takes place.

In addition to our basic disagreement with the need for S. 3042, several of its provisions appear to require clarification. We feel confident that the concept "underground uses of nuclear energy" was not intended to refer to reactors or radioisotopic device testing. Beyond that, however, it is difficult for us to ascertain precisely what activities are intended to be covered.

Section 2(a) may also require clarification. It provides that the majority of the fifteen member Commission shall be selected on the basis of "recognized expertise in the fields of air, water, and land pollution" while the remainder shall be selected on the basis of "special knowledge and expertise in disciplines related to the human and physical environment." One could reasonably view "disciplines related to the human and physical environment" as including "expertise in the fields of air, water, and land pollution" making it difficult to ascertain precisely what expertise the Commission members are expected to possess.

Section 3(c)(2) proscribes the taking of action on any report required by any other provision of law relevant to the use of nuclear energy for excavation purposes until the proposed Commission has transmitted its final report to the Congress and the President. It is our understanding that the only such report upon which action is proscribed by this provision is that of the Canal Study Commission, which, pursuant to Public Law 88–609, as amended, is required to make its recommendations to the President no later than December 1, 1970.

In conclusion, we believe that our existing comprehensive and objective safety program is working effectively, and that there exists both Executive and Congressional authority which, if needed, could be invoked to conduct the kind of study envisaged by S. 3042. Accordingly, the Atomic Energy Commission does not believe that establishment of a new and essentially redundant entity would be in the national interest.

The Bureau of the Budget has advised that there is no objection to the presentation of these comments from the standpoint of the Administration's program.

Cordially,

C. E. LARSON,
Acting Chairman.

ENCLOSURE A

NON-AEC EXPERTS ON ENVIRONMENTAL EFFECTS OF UNDERGROUND NUCLEAR EVENTS

I. *Nevada Operations Office (NVO) Standing Panel of Safety Consultants*

Dr. George B. Maxey (Hydrology), University of Nevada, Reno, Nevada
Mr. Thomas F. Thompson (Geological Engineering), Burlingame, California

Mr. Lewis G. von Lossberg (Hydrogeology), S. T. Powell and Associates, Baltimore, Maryland
Mr. Stanley D. Wilson (Soil Mechanics), Shannon and Wilson, Seattle, Washington
Dr. Don U. Deere (Rock Mechanics), University of Illinois, Urbana, Illinois
Dr. Lydik S. Jacobsen (Structural Response), Santa Rosa, California
Dr. Nathan M. Newmark (Structural Response), University of Illinois, Urbana, Illinois
Dr. James T. Wilson (Seismology), University of Michigan, Ann Arbor, Michigan
Dr. Carl Kisslinger (Geophysics), St. Louis University, St. Louis, Missouri
Dr. William G. Van Dorn (Oceanography), Scripps Institute of Oceanography, La Jolla, California
Dr. Leo K. Bustad (Radiobiology), University of California, Davis, California
Dr. Vincent Schultz (Ecology), Washington State University, Pullman, Washington
Leonard A. Sagan, M.D. (Medical), Palo Alto Medical Center, Palo Alto, California

II. *NVO Ad Hoc Group of Safety Consultants*

Dr. Clarence R. Allen (Geology/Geophysics), California Institute of Technology, Pasadena, California
Dr. Perry Byerly (Seismology), Prof. Emeritus, University of California, Berkeley, California
Dr. George Plafker (Geology), USGC, Menlo Park, California
Dr. Jack E. Oliver (Seismology), Lamont-Doherty Geologic Observatory, Palisades, New York
Rev. William J. Stauder, S.J. (Seismology), St. Louis University, St. Louis, Missouri
Dr. Lester Machta (Meteorology), ESSA/ARL, Silver Spring, Maryland
Dr. Kenneth M. Nagler (Meteorology), ESSA/Weather Bureau, Silver Spring, Maryland
Dr. Everett F. Cox (Air Blast Phenomenology), Benton Harbor, Michigan

III. *Other Experts Involved in Conduct of NVO Effects Evaluation Program*

Dr. Paul T. Tueller (Vegetation and Soil Response to Underground Detonations), The University of Nevada at Reno College of Agriculture, Reno, Nevada
Dr. Richard S. Davidson (Ecology Research), Battelle Laboratories, Columbus, Ohio
Dr. Bruce McAlister (Marine Ecology and Oceanography), U.S. Bureau of Commercial Fisheries, Seattle, Washington
Dr. R. L. Burgner (Marine Ecology and Oceanography), University of Washington, Fisheries Research Institute, Seattle, Washington
Dr. J. N. Neuhold (Freshwater Ecology), Utah State University
Dr. D. D. Koob (Freshwater Ecology), The Ohio State University Institute of Polar Studies
Dr. K. R. Everett (Geomorphology), The Ohio State University Institute of Polar Studies
Dr. E. E. C. Clebsch (Plant Ecology), University of Tennessee
Dr. Francis S. L. Williamson (Avian Ecology), Johns Hopkins University
Dr. A. H. Seymour (Radiometric and Elemental Analysis), University of Washington, Seattle, Washington
Dr. William S. Twenhofel (Internal Geological Studies), U.S. Geological Survey, Denver, Colorado
Mr. Samuel West (Hydrological Studies), U.S. Geological Survey, Denver, Colorado
Dr. Paul R. Fenske (Ground Water Contamination), Isotopes, Inc., Palo Alto, California
Mr. Alan E. Peckham (Central Nevada Studies), University of Nevada, Desert Research Institute, Las Vegas, Nevada
Mr. Kenneth W. King (Ground Motion Measurements), ESSA/U.S. Coast and Geodetic Survey, Special Projects Party, Las Vegas, Nevada
Mr. O. Allen Israelson (Ground Motion Evaluation), Environmental Research Corporation, Alexandria, Virginia
Dr. John A. Blume (Structural Dynamic Effects), John A. Blume & Associates, San Francisco, California

Dr. Paul L. Russell (Mine and Wall Studies), U.S. Bureau of Mines, Denver Mining Research Center, Denver, Colorado
Dr. Lester Machta (Fallout Models, Atmospheric and Cratering Events), ESSA Air Resources Laboratory, Silver Spring, Maryland
Mr. Philip W. Allen (Radiation Models, Underground and Plowshare Events) (Weather and Trajectory Prediction Studies), ESSA Air Resources Laboratory, Las Vegas, Nevada
Dr. Paul MacCready (Long Range Diffusion Study), Meteorology Research, Inc., Altadena, California
Mr. J. E. McNeil (Tsunami), A. C. Electronics/Defense Research Laboratory, Santa Barbara, California
Dr. B. LeMehaute (Tsunami), Tetra Tech, Pasadena, California
Dr. William M. Adams (Tsunami), University of Hawaii, Honolulu, Hawaii
Mr. Kenneth W. King (Seismic Measurements), ESSA/U.S. Coast and Geodetic Survey, Special Projects Party, Las Vegas, Nevada
Dr. Donald Tocher, ESSA/Earthquake Mechanisms Laboratory, San Francisco, California
Dr. Maurice W. Major, Colorado School of Mines, Boulder, Colorado
Dr. Alan S. Ryall, University of Nevada, Mackay School of Mines, Seismological Laboratory, Reno, Nevada
Mr. Frank A. McKeown, U.S. Geological Survey, Denver, Colorado
Dr. John H. Healy, USGS Earthquake Laboratory, Menlo Park, California
Dr. Stewart Smith, California Institute of Technology, Pasadena, California
Dr. Leon Knopoff, University of California, Los Angeles, California

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(Department of Defense:)

General Counsel of the Department of Defense,
Washington, D.C., November 25, 1969.

Hon. Jennings Randolph,
Chairman, Committee on Public Works,
U.S. Senate, Washington, D.C.

Dear Mr. Chairman: Reference is made to your request for the views of the Department of Defense with respect to S. 3042, 91st Congress, a bill "To provide for a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation and for other purposes."

Inasmuch as the subject matter of the bill is within the purview of the Atomic Energy Commission, the Department of Defense defers to that agency as to the merits of S. 3042.

The Bureau of the Budget advises that, from the standpoint of the Administration's program, there is no objection to the presentation of this report for the consideration of the Committee.

Sincerely,

L. Niederlehner,
Acting General Counsel.

(Executive Office of the President:)

Executive Office of the President,
Office of Science and Technology,
Washington, D.C., November 26, 1969.

Hon. Jennings Randolph,
U.S. Senate,
Washington, D.C.

Dear Senator Randolph: Thank you for the opportunity to comment on Senate Bill S. 3042 to provide a study and evaluation of environmental underground uses of nuclear energy. I think the matters addressed by this bill are extremely important and I would certainly support the view that no peaceful applications of atomic energy should be undertaken for excavation or other purposes which constitute serious danger to the environment.

As you know, this is not a new subject. For many years the biomedical program of the Atomic Energy Commission has attempted to analyze biological and other environmental effects associated with nuclear explosions. In addition,

other agencies of the Federal Government including the Federal Radiation Council; Department of Health, Education and Welfare; the Department of the Interior; the Executive Office of the President; and many others have reviewed these matters utilizing the services of scientists and engineers from within the government and from the scientific community at large. Their results have at times been supporting and at other times been critical of AEC's planned programs. I would like to state clearly that, as far as I am aware, these studies have been conducted conscientiously and without bias in favor of these programs. In cases with which I am familiar the studies incorporated the most complete and up to date information available at the time that they were done. A number of important changes in program direction have been occasioned within the Atomic Energy Commission as a result of these reviews and analyses need to be continued by a wide variety of agencies and groups both within and without the government until such time as we are certain that planned programs do not constitute a hazard.

I would not recommend the establishment of a Presidential Commission for the purpose proposed by S. 3042 on two grounds.

First, the assumption underlying the proposal seems to be that such a commission could provide an authoritative and final conclusion about the environmental hazards associated with nuclear explosions. However, our previous experience with research and development in this area suggests that the assumption is not so, that new problems and new solutions continually emerge. Thus, establishment of this proposed commission could have the unintended consequence of retarding the study of environmental effects of nuclear explosion which should continue to be carried on in a number of quarters. Second, the limited membership of the proposed Commission could result in overlooking some important technical aspects of the problem of nuclear excavation with a resulting gap in the findings and an inadequacy in the solutions proposed. This deficiency, too, can best be remedied by continuing studies carried out by a number of different individuals and groups.

I suggest that ample mechanisms are already available to study and investigate environmental effects of the type referred to in S. 3042. These are available to the Executive Branch, to the Congress, and other, different mechanisms are available to the general public. These certainly include: congressional hearings; advisory groups from outside the government assembled under the President's Advisory Committee and/or Government Agencies; advisory and task groups assembled from the Federal Government under the Federal Council of Science and Technology, the Federal Radiation Council or operating agencies; the National Academy of Sciences, who have always responded to the Congress and the Executive Branch when asked; professional and scientific societies, who have always been willing to conduct open forums for discussion quite apart from Federal Government influence; and the many citizens groups who have become increasingly vigilant about environmental matters. In summary, I support the intent of S. 3042 to study carefully the environmental effects of the underground nuclear explosion programs but conclude that a Presidential Commission is not the best way to accomplish this.

Very truly yours,

Lee A. DuBridge, *Director.*

[S.J. Res. 108, 91st Cong., first sess.]

JOINT RESOLUTION To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbances

Resolved by the Senate and House of Representatives of the United States of America in Congress assembled, That this joint resolution may be cited as the "National Commission on Nuclear and Seismic Safety".

ESTABLISHMENT OF COMMISSION

Sec. 2. There is hereby established a National Commission on Nuclear and Seismic Safety (hereinafter referred to as the "Commission").

MEMBERSHIP

Sec. 3. (a) The Commission shall be composed of fifteen members to be appointed by the President from among representatives of nuclear physics, geo-

physics, seismology, hydrology, oceanography, structural engineering, architecture, urban planning, economics, biology, and medicine.

(b) No member of the Commission at the time of his appointment shall be in the employ of the Government, or under contract with the Government, or otherwise engaged in research or consultation for the Government as the employee of a private business organization under contract with the Government: *Provided, however*, That this subsection shall not operate as a bar to the appointment of a person, not a Government employee, whose work under such contract is not directly related to the functions of the Commission as set forth below.

(c) Any vacancy in the Commission shall not affect its powers.

(d) The President shall designate one of the members to serve as Chairman and one to serve as Vice Chairman of the Commission.

(e) Eight members of the Commission shall constitute a quorum.

DUTIES OF THE COMMISSION

SEC. 4. (a) The Commission shall undertake a comprehensive investigation and study of the implications of underground and other nuclear detonations including but not limited to the following: implications for earthquakes, other seismic disturbances both subterranean and submarine, ecological contamination and waste, and damage to existing structures.

(b) The Commission shall transmit to the President and to the Congress such interim reports of its findings as it deems necessary or advisable.

(c) The Commission shall transmit to the President and to the Congress not later than one year after the first meeting of the Commission a final report containing a detailed statement of the findings and conclusions of the Commission, together with its recommendations, including such recommendations for legislation and administrative action as it deems advisable.

POWERS OF THE COMMISSION

SEC. 5. (a) The Commission or, on the authorization of the Commission, any subcommittee or members thereof, may, for the purpose of carrying out the provisions of this joint resolution, hold such hearings, take such testimony, and sit and act at such times and places as the Commission deems advisable. Any member authorized by the Commission may administer oaths or affirmations to witnesses appearing before the Commission or any subcommittee or members thereof.

(b) Each department, agency, and instrumentality of the executive branch of the Government, including independent agencies, is authorized and directed to furnish to the Commission, upon request made by the Chairman or Vice Chairman, such information as the Commission deems necessary to carry out its functions under this joint resolution.

(c) Subject to such rules and regulations as may be adopted by the Commission, the Chairman shall have the power to—

(1) appoint and fix the compensation of an executive director, and such additional staff personnel as he deems necessary, without regard to the provisions of title 5, United States Code, governing appointments in the competitive service, and without regard to the provisions of chapter 51 and subchapter III of chapter 53 of such title relating to classification and General Schedule pay rates, but at rates not in excess of the maximum rate for GS-18 of the General Schedule under section 5332 of such title, and

(2) procure temporary and intermittent services to the same extent as is authorized by section 3109 of title 5, United States Code, but at rates not to exceed $50 a day for individuals.

(d) The Commission is authorized to enter into contracts with Federal or State agencies, private firms, institutions, and individuals for the conduct of research or surveys, the preparation of reports, and other activities necessary to the discharge of its duties.

COMPENSATION OF MEMBERS

SEC. 6. Members of the Commission shall receive compensation at the rate of $100 per day for each day they are engaged in the performance of their duties as members of the Commission and shall be entitled to reimbursement for travel, subsistence, and other necessary expenses incurred by them in the performance of their duties as members of the Commission.

APPROPRIATIONS AUTHORIZED

SEC. 7. There are hereby authorized to be appropriated such sums as may be necessary to carry out the provisions of this joint resolution.

TERMINATION

SEC. 8. On the ninetieth day after the date of submission of its final report to the President, the Commission shall cease to exist.

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., November 14, 1969.

Hon. EDMUND MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, New Senate Office Building, Washington, D.C.

DEAR MR. CHAIRMAN: In view of the fact that you are the Chairman of the Subcommittee on Air and Water Pollution and having been informed that you are planning to hold hearings next week on Senator Gravel's bill, S. 3042, I am sending you a copy of the letter which I have sent as of this date to Senator Gravel. I am also enclosing comments from various executive agencies on S.J. Res. 108, another bill introduced by Senator Gravel which was referred to the Joint Committee on Atomic Energy.

The similarities of the two bills make the comments of the executive agencies pertinent not only to S.J. Res. 108 but also to S. 3042.

Sincerely,

CHET HOLIFIELD.

Enclosures.

CONGRESS OF THE UNITED STATES,
JOINT COMMITTEE ON ATOMIC ENERGY,
Washington, D.C., November 14, 1969.

Hon. MIKE GRAVEL,
Old Senate Office Building,
Washington, D.C.

DEAR SENATOR GRAVEL: I note that Senator Muskie's subcommittee is going to have hearings on your bill, S. 3042. My staff has examined this bill and they inform me that the purpose and intent of S. 3042 is almost identical with S.J. Res. 108.

Our Committee received your bill, S.J. Res. 108 on May 8 and on May 17 we forwarded it to the different executive agencies for comment, as is the custom of our Committee. I regret that I have not as yet received comments from all of the agencies—specifically the Departments of Commerce and Health, Education, and Welfare. However, I have received comments from a number of agencies, and I am forwarding you copies of those I have received. The letters indicate their opposition to the approach of S.J. Res. 108. Because of this opposition I am also sending copies to the Chairman of the Subcommittee, our mutual friend, Senator Muskie. When I receive the comments from the two agencies mentioned above, I will also forward a copy to you.

Sincerely yours,

CHET HOLIFIELD.

(Executive Office of the President:)

EXECUTIVE OFFICE OF THE PRESIDENT,
OFFICE OF SCIENCE AND TECHNOLOGY,
Washington, D.C., October 27, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D.C.

DEAR MR. HOLIFIELD: This is in reply to your request for advice on the proposal contained in Senate Joint Resolution 108 to set up a commission to study nuclear and seismic safety associated with underground nuclear testing.

I feel that such a commission is both unnecessary and cumbersome. To the extent that public hazards are involved prompt response is required. The past history of commissions suggests that they are not suitable mechanisms to respond promptly. There exists, within the Federal Government, adequate alternative mechanisms for investigating problems of this sort. I would like to remind the committee of some of the steps which have already been taken using existing mechanisms:

(a) The advisory groups of the Atomic Energy Commission for site safety, seismic and other hazards have been very active and very diligent. It is my personal opinion that these groups of outside advisers have in no sense attempted to whitewash the AEC's position on safety of atomic energy programs. I do not suggest that it is enough to rely solely on this mechanism but I would like to state my confidence in the non-government scientists who perform this activity.

(b) An ad hoc panel of the President's Science Advisory Committee under Dr. Kenneth Pitzer reviewed this subject a year ago and their report has recently been made public by the Atomic Energy Commission. A copy is attached.

(c) With the full cooperation of the Atomic Energy Commission an open scientific symposium was held at the American Geophysical Union meetings in Washington last April in which all sides of the seismic hazard question were presented and freely discussed. Similar discussions took place with the cooperation of the Atomic Energy Commission in several other scientific forms.

I think the subject of safety and possible environmental hazards associated with our atomic energy programs, whether military or civilian, is sufficiently important that pertinent information should be gathered and made public, and that experienced specialists should be invited to comment. In the case of the just completed test in Amchitka, we should first examine the scientific observations obtained in the extensive observational program maintained for the test. For those areas of concern where the observations exactly match the expectations of previous studies it may not be necessary to consider new review mechanisms incorporating those specialties. In cases where the observations produced any unexpected or otherwise new results, prompt review of the potential hazards is indicated. This can be done within existing mechanisms in my opinion including:

(a) A panel of scientists under the auspices of my office, the Environmental Quality Council, the National Academy of Sciences, or other appropriate bodies.

(b) Hearings held by the Joint Committee or other appropriate committees of Congress might enable the opinion of all relevant experts and interested parties to be heard and the matter to be submitted to public discussion. The Chairman of the Atomic Energy Commission, the Director of the U.S. Geological Survey, and my office would be glad to suggest names of suitable experts.

(c) Relevant professional societies might well be invited to hold symposia on the subject in whatever ways they find useful. I feel certain that government agencies involved, including the Atomic Energy Commission, would be pleased to cooperate in any reasonable efforts in this direction.

I hope I have communicated to you my feeling that the question is an important one and that I feel that all reasonable precautions should be taken, but that the commission seems to represent an unnecessary and cumbersome mechanism.

Very truly yours,

LEE A. DUBRIDGE, *Director*.

(Department of Defense:)

GENERAL COUNSEL OF THE DEPARTMENT OF DEFENSE,
Washington, D.C., October 30, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy, Congress of the United States, Washington, D.C.

DEAR MR. CHAIRMAN: Reference is made to your request for the views of the Department of Defense on S. J. Res. 108, 91st Congress, a Resolution "To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbances."

S. J. Res. 108 would establish a National Commission on Nuclear and Seismic Safety to study and evaluate the relationship between underground nuclear detonations and seismic disturbances. The Commission would consist of 15 members

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appointed by the President and would undertake a comprehensive investigation and study of the implications of underground and other nuclear detonations including, but not limited to: implications for earthquakes, other seismic disturbances both subterranean and submarine, ecological contamination and waste, and damage to existing structures.

The Department of Defense generally supports the broad objective of S. J. Res. 108 to improve understanding of the relationship between underground nuclear detonations and seismic disturbances. However, it believes that the means proposed by S. J. Res. 108 will not attain this objective and that the proposed commission is unnecessary. Therefore, the Department of Defense is opposed to enactment of S. J. Res. 108.

The relationship involved has been, and continues to be, studied extensively by the Department of Defense, the Atomic Energy Commission, the Geological Survey, the Environmental Science Services Administration, and other agencies. These agencies utilize private contractors as well as capable Government personnel in this area. The technical views and judgments of both government and non-government scientists are subject to critical examination and challenge by fellow scientists.

While extensive data are available for review, they have been exhaustively examined by competent scientists. It is very doubtful that reexamination of these same data will produce any new or conclusive results. The Department of Defense believes very strongly that further advances in understanding the relationship between underground nuclear detonations and seismic disturbances will come only from the development and analysis of new information. Current programs should provide for these further advances and, in effect, accomplish the objective of the draft Resolution.

The Bureau of the Budget advises that, from the standpoint of the Administration's program, there is no objection to the presentation of this report for the consideration of the Committee.

Sincerely,

L. NIEDERLEHNER,
Acting General Counsel.

(Executive Office of the President:)

EXECUTIVE OFFICE OF THE PRESIDENT,
OFFICE OF EMERGENCY PREPAREDNESS,
Washington, D.C., October 31, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D.C.

DEAR MR. CHAIRMAN: This is in reply to your request for comments from this Agency concerning S.J. Res. 108, 91st Congress, cited as: "National Commission on Nuclear and Seismic Safety".

The resolution would authorize the establishment of a National Commission on Nuclear and Seismic Safety to conduct a comprehensive investigation and study of the implications of underground and other nuclear detonations with respect to earthquakes and other seismic disturbances, ecological contamination and waste, and damage to existing structures. The Commission would report to the President and the Congress.

It is our understanding that the Atomic Energy Commission, the Department of Defense, and other agencies have conducted a substantial amount of research along the lines of the work contemplated by S.J. Res. 108. Consequently, we are unable to advise you as to whether there is a present need for a comprehensive investigation of this type. Accordingly, we defer to the views of those agencies and of the Bureau of the Budget as to whether such a study should be undertaken at this time, and, if so, whether a Commission should be established for the purpose or whether it could be more effectively accomplished by existing agencies of the Government or by a public or private research organization.

From the standpoint of the Administration's program, the Bureau of the Budget advises that it has no objection to the submission of this report.

Sincerely,

G. A. LINCOLN, *Director*.

(Atomic Energy Commission:)

ATOMIC ENERGY COMMISSION,
Washington, D.C., November 6, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States.

DEAR MR. HOLIFIELD: This letter is in response to your request for our comments on Senate Joint Resolution 108 ("To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbances").

The AEC supports the objectives sought by S.J. Res. 108—that is, that planned underground nuclear detonations should be subject to comprehensive and objective safety reviews by experts in the pertinent disciplines. We believe that these objectives safety reviews by experts in the pertinent disciplines. We believe that these objectives have been and are being achieved in the AEC nuclear testing program through our established regular procedures, as supplemented from time to time by special studies and reviews which are designed to assure that no test is undertaken without adequate provision for all aspects of safety.

Pursuant to this AEC program, there have been extensive studies concerning all the basic safety aspects of undeground nuclear detonations which serve as a basis for the specific safety studies and reviews for each test. (These studies are summarized in a recently published report, "Safety of Underground Nuclear Testing." This report has been submitted to the JCAE and is available to the public as TID-24996. For your convenience, a copy is enclosed.) To assure the objectivity of the conclusions upon which the AEC bases its safety decisions, the AEC has consistently fostered reviews by experts with outstanding professional reputations from Government, universities, and private industry. Some significant features of our overall efforts are:

a. There is an established panel of Safety Consultants which conducts reviews, makes evaluations, and advises on all aspects of the safety of the nuclear testing program. Members include experts in such disciplines as geology, hydrology, soil and rock mechanics, and structural engineering. Recently the National Academy of Sciences has provided additional nominees to expand the panel to include such disciplines as seismicity, tsunamis, and ecology.

b. Since October 1968 there has been an Ad Hoc Committee on Seismicity, consisting of several of the nation's foremost experts in seismology, conducting a review of any possible interactions between underground nuclear detonations and natural seismic phenomena. This Committee reviewed all evidence on seismic aftershocks from testing, and recommended investigations be conducted during the BENHAM event of all seismic phenomena which might accompany high yield tests. These investigations were carried out and the Committee is now reviewing these data as well as the proposed seismic data recording program for Amchitka.

c. We have involved the National Academy of Sciences and its Polar Research Committee in the review of AEC plans for protecting the Amchitka National Wildlife Refuge during the planned nuclear testing there.

d. The AEC encouraged the American Geophysical Union, a respected forum for geophysicists and geologists, to devote an entire day's program at their recent Washington, D.C. meeting to presentations on the seismic effects of large underground explosions.

We note that the resolution precludes the appointment to the proposed Commission of persons who are associated with related work of the AEC as contractors or contractor employees, or who are employed by the Government. We believe that it would be very difficult to find enough people with the required program knowledge who are not associated with some phase of such AEC work either as research workers or consultants. We believe that objectivity can be expected from the professional scientists and engineers who serve as safety consultants and seismic advisors to the AEC under existing arrangements. It should be noted that their responsibilities relate solely to safety responsibilities and they have no other AEC program responsibility.

The AEC utilizes the best available expertise in studying the safety of underground nuclear tests and we would therefore consider it unlikely that reexamination of existing data by yet another group will provide any new insights. In fact,

seismology is a relatively new and developing field with only a limited number of available experts. There should, therefore, be a real concern that the creation of a new Commission restricted to persons not associated with the Government will lead to its dependence on few or less knowledgeable consultants. The alternative would be to deplete the number of competent experts presently advising the AEC and its field test organization.

The approach being taken by the AEC emphasizes the conduct of new experiments to produce new data, rather than reviews of old data which have been very comprehensively analyzed and interpreted. Admittedly this process is a slow one. The development of seismology from its present observational state into a science with an ability to accurately predict earthquakes and the interrelations between various types of ground shocks necessitates such a careful scientific approach. Progressing to a point of demonstrable cause and effect relationships or even statistical inference takes time, but such careful work leads to a steady evolution of understanding concerning the nature of earthquakes. For this reason it is difficult to see how the proposed Commission could in one year throw much further light on the subject.

While we believe that we can rely fully on the adequacy and objectivity of our safety reviews, there is still a further assurance in the requirement that Presidential approval of these tests be obtained. If incident to such approval, further scientific study and review should be desired, the President has readily available his Science Adviser and other technical resources within the various Executive agencies. In fact, the Under Secretaries Committee of the National Security Council including representatives from State, Defense, ACDA, NSC and the Science Adviser together with technical assistance as necessary from USGS, USC&GS, PHS and ESSA reviews the entire underground test program annually and, in addition, it reviews the program again on a quarterly basis and gives individual review to events of a special nature. The committee's reviews are made from the standpoint of all factors (e.g., political, national security requirements, conformity with international agreements, public safety, etc.), and in light of all these considerations including adequacy of safety measures the committee makes its recommendations to the President.

Should it be concluded that there is widespread public concern about the safety of the AEC's underground testing program and that some special study and review is warranted, there are also existing Congressional powers for this purpose. I need not tell you, Mr. Chairman, that the Joint Committee on Atomic Energy has in analogous situations when questions were raised about atomic energy matters promptly conducted reviews and when necessary hearings to assure the adequacy of the overall Government program.

Since we believe that our existing comprehensive and objective safety program is working effectively, and that there is in existence both Executive and Congressional authority which, if needed, could be invoked to conduct the kind of study envisaged by Senate Resolution 108, the AEC does not believe that establishment of a new and essentially redundant entity would be in the national interest.

The Bureau of the Budget has advised that there is no objection to the presentation of these comments from the standpoint of the Administration's program.

Cordially,

GLENN T. SEABORG, *Chairman.*

(Department of the Interior:)

U.S. DEPARTMENT OF THE INTERIOR,
Washington, D.C., November 13, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D.C.

DEAR MR. CHAIRMAN: Your Committee has requested the views of this Department on S.J. Res. 108, a joint resolution "To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbances."

We recommend that the resolution not be adopted.

S.J. Res. 108 would establish a temporary National Commission on Nuclear and Seismic Safety which would investigate and report to the President on the implications of underground and nuclear testing. The Commission would be com-

posed of 15 nongovernmental members to be appointed by the President from among representatives of nuclear physics, geophysics, seismology, hydrology, oceanography, structural engineering, architecture, urban planning, economics, biology, and medicine. The Commission's study is to include but not be limited to the following areas of inquiry: implications for earthquakes, other seismic disturbances both subterranean and submarine, ecological contamination and waste, and damage to existing structures. It is to transmit a final report to the President not later than one year after its first meeting containing a detailed statement of the findings and conclusions of the Commission, together with its recommendations, including necessary legislation and administrative action. The Commission shall cease to exist ninety days after it submits its final report.

We agree that the effects of underground nuclear detonations should be extensively investigated. This Department has repeatedly urged that a full assessment of the environmental impact of such detonations be made and that we be given the opportunity for early and continuing review of such projects.

However, we do not feel that there is need for a separate committee on nuclear and seismic safety. The competency in these fields primarily is in the Atomic Energy Commission, the Geological Survey, and to some extent the Environmental Science Services Administration. There may be a need for accelerated investigative and action programs by these agencies and for more positive direct coordination but this should best be achievable through direct channels rather than a separate commission.

If a specific study is to be made, we believe it should be made by the operating agencies. It should include whatever external input as is needed to assure that all factors are considered. All of the disciplines and capabilities set forth in Section 3 of S.J. Res. 108 exist within the directly involved Federal agencies and the use of this operational expertise should be the most effective way of making such a study.

Section 3(a) of the joint resolution lists the fields of study which should be represented on the commission, but it omits several fields of study which we think essential to the protection of the environment and the interests of this Department. Representatives, should also be chosen from the fields of ecology, water and air pollution, climatology, geology, and soils engineering. Expertise in all these fields, and perhaps in others as well, should be available to the commission.

Also, we believe that the resolution is shortsighted in barring government employees from membership on the proposed commission. Such a provision would eliminate many of the most knowledgeable persons from consideration for service on the commission. We do not believe that government personnel should be prohibited from serving on such a commission.

The Bureau of the Budget has advised that there is no objection to the presentation of this report from the standpoint of the Administration's program.

Sincerely yours,

RUSSELL E. TRAIN,
Under Secretary of the Interior.

(Department of Commerce:)

GENERAL COUNSEL OF THE DEPARTMENT OF COMMERCE,
Washington, D.C., December 8, 1969.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
Congress of the United States, Washington, D.C.

DEAR MR. CHAIRMAN: This is in further reply to your request for the views of this Department concerning S.J. Res. 108, a joint resolution "To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbance," to be cited as the "National Commission on Nuclear and Seismic Safety."

S.J. Res. 108 would establish a National Commission on Nuclear and Seismic Safety, composed of 15 members appointed by the President. The Commission would undertake a comprehensive investigation and study of the implications of underground and other nuclear detonations on such things as earthquakes, subterranean and submarine seismic disturbances, ecological contamination and waste, and damage to existing structures. The Commission would submit a report on its findings and recommendations to the President and the Congress within one year. Executive agencies would be directed to furnish to the Com-

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mission such information as the Commission deems necessary to carry out its functions. The Commission would be empowered to enter into contracts with Federal and State agencies, private firms, institutions, and individuals for the conduct of research or surveys. Subject to rules adopted by the Commission, the Chairman would be authorized to appoint an executive director, and such additional staff personnel as he deems necessary. The resolution authorizes to be appropriated such sums as may be necessary to carry out its provisions.

By letter dated October 27, 1969, Dr. Lee A. DuBridge, Director of the Office of Science and Technology, commented to your Committee on S.J. Res. 108. Dr. DuBridge expressed with respect to the Commission proposed in S.J. Res. 108 his feeling that such a Commission is both unnecessary and cumbersome. Also, he stated there exists, within the Federal Government, adequate alternative mechanisms for investigating problems of the sort assigned to the proposed Commission. He specified some of the steps which have already been taken using existing mechanisms. Also, he noted other existing mechanisms that could be utilized when deemed necessary.

This Department supports fully the comments of Dr. DuBridge concerning S.J. Res. 108.

We have been advised by the Bureau of the Budget that there would be no objection to the submission of our report to the Congress from the standpoint of the Administration's program.

Sincerely,

JAMES T. LYNN,
General Counsel.

(Department of Health, Education, and Welfare:)

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE,
Washington, D.C.

Hon. CHET HOLIFIELD,
Chairman, Joint Committee on Atomic Energy,
U.S. Capitol, Washington, D.C.

DEAR MR. CHAIRMAN: This letter is in response to your request of May 17, 1969, for a report on S.J. Res. 108, a Resolution "To provide for a study and evaluation of the relationship between underground nuclear detonations and seismic disturbances."

The resolution would provide for the establishment of a National Commission on Nuclear and Seismic Safety to undertake a comprehensive investigation and study of the implications of underground and other nuclear detonations such as implications as to earthquakes, ecological contamination and waste, and damage to structures. The Commission would transmit interim reports to the President and Congress as deemed necessary or advisable and a final report containing a detailed statement of findings, conclusions and recommendations not later than one year after its first meeting.

We are in accord with the objectives of the resolution. However, we do not believe that the establishment of such a Commission to study and report on the problems of underground nuclear detonations and seismic disturbances is the most expeditious or appropriate mechanism to accomplish these objectives. As an alternate approach to the establishment of the Commission, we would prefer the use of existing scientific organizations such as the President's Science Advisory Committee or the National Academy of Science. We believe that such a group of experts could analyze all factors related to public health and safety and environmental quality and inform the public on these matters. We believe such a group could investigate the potential seismic and environmental effects that may result from detonations conducted at the Nevada Test Site and at other locations to assure the protection of the public. This Department through its Bureau of Radiological Health has conducted environmental surveillance of such detonations for several years at the Nevada Test Site and other locations in a support role for the Atomic Energy Commission. Experience gained in this regard should be of value to the study of these questions, and we could provide data arising therefrom.

We also believe that the scientific findings of the study group should have the objective of establishing as clearly as possible that the detonations can be safely executed without adversely affecting the ecology, underground water supplies,

and surface structures. The study group could also assure that the health and safety of the public from long-term as well as immediate effects will receive the highest priority in the execution of all underground and other nuclear detonations.

We therefore support the objectives of this resolution but believe that they could be better accomplished by existing scientific organizations.

We are advised by the Bureau of the Budget that there is no objection to the presentation of this report from the standpoint of the Administration's program.

Sincerely,

ROBERT H. FINCH,
Secretary.

STAFF BRIEFING PAPER

BACKGROUND

Since 1957, the Atomic Energy Commission has invested about $135 million in the Plowshare program to develop a technology for using nuclear explosives for excavation and other peaceful applications. These applications include construction (e.g., canals, harbors, dams), industrial development of natural resources (e.g., gas, oil, minerals, water), and scientific experiments (e.g., neutron physics, heavy element production, seismology). Several specific applications, in progress or under feasibility study, are discussed herein.

The stimulus for Plowshare is primarily economic. For example, the cost for a ton of TNT is $460.00. The projected cost of a 10 kiloton (10,000 tons) thermonuclear (fusion) device is approximately $35.00 per "energy-equivalent" ton while the cost of a 2 megaton (2,000,000 tons) fusion device is approximately $.30 per ton.

Another important consideration is an advantage in emplacement size for nuclear explosives. For example, the volume required for 2 megatons of TNT is about one million cubic meters compared to a volume of less than 10 cubic meters for an equivalent nuclear device.

The Subcommittee on Air and Water Pollution is concerned with the environmental effects of this use of nuclear energy. The environmental considerations include (1) the effects on public health and welfare caused by pollution of the environment, (2) the effects on terrestrial, marine, and fresh water ecosystems, (3) the distribution and transport of the produced radioactive venting to the atmosphere and fallout, by hydrologic transport and by product contamination and, associated with these, (4) the effects caused by propagated seismic energy including tidal waves (tsunamis) and earth tremors. (The Subcommittee is not specifically concerned with this latter aspect at this time.)

The concomitant production of hazardous amounts of radioactive materials during nuclear detonations has led to international restraints in the testing of nuclear explosives in the atmosphere and on the surface of the earth. Pressure is being brought to bear from the industrial community to develop nuclear explosives for peaceful purposes, separate from purposes of national security. As this effort becomes successful there is a need for discussion of the social benefits and costs to be derived from this technology.

POTENTIAL ENVIRONMENTAL HAZARDS ASSOCIATED WITH UNDERGROUND USE OF NUCLEAR ENERGY FOR EXCAVATION

Both radiological and non-radiological hazards result from the underground uses of nuclear explosives. Of immediate concern are the short-term radiological exposures resulting from venting, particularly when cratering applications are involved. Additionally, radioactive materials may be deposited in the vicinity of the site and in the ground water passing over or through the rubble.

In applications involving resource extraction of materials such as gases, oil, and mineral ores, the extracted resource may be contaminated. This would be a potential hazard if the radioactivity accompanied the resource from the extraction phase of the operation through refining and processing to consumer use.

The first source is the fission of uranium or other fissionable materials which are used as the trigger for fusion explosives. Of particular radio-biological concern are the long-lived fission products of cesium-137 and strontium-90. Also of concern are carbon-14 and iodine-131 and krypton-85.

Cesium-137, strontium-90 and iodine-131 are of concern because of their rapid incorporation in the food-chain of man and the resultant exposure to both bone and the blood forming organs for strontium-90 and man's total body for cesium-137. Strontium-90 is deposited in bone and results in exposure of blood forming organs. Thyroid exposures are of concern for iodine-131.

Second, thermo-nuclear reaction products are produced from the fusion explosives. Of primary concern is tritium which amounts to approximately 7,000 to 50,000 curies per kiloton.

Many experts are of the opinion that the limit of our ability to utilize nuclear sources of energy for both power and other purposes will be determined by the allowable global inventory of long-lived radionuclides of tritium, carbon-14 and krypton-85.

Krypton-85 is a chemically inert gas with radiological half-life of 10.3 years. It can freely migrate through the atmosphere and in time will assume a global distribution. For these reasons it presents a potential long-term radiation hazard of international significance.

Although chemically active, tritium and carbon-14 will achieve global distributions, also. These elements mix freely with their nonradioactive counterparts, hydrogen and carbon, in the total environment.

Tritium is incorporated into water molecules as hydrogen (tritiated water) which enters the global water cycle through condensation and evaporation. Additionally, tritiated water enters biological life-cycles. It is then ingested in food and incorporated as water in body cells.

Carbon-14 is incorporated into many carbon compounds, including carbon dioxide which escapes as a gas into the atmosphere. It becomes globally mixed with non-radioactive carbon dioxide and enters into the life-cycle of plants which may end up in man's food chain.

Third, activation products are produced when the material surrounding the explosion absorbs neutrons. The particular activation products depend upon the composition of the surrounding materials; however, iron-55, calcium-45, and cobalt-60 are of particular concern.

Iron-55 is concentrated by aquatic and marine organisms which serve as food supplies. After intake iron-55 is deposited in bone and red blood cells. For these reasons it is of radiobiological concern.

Calcium-45 is radiobiologically of concern because of deposition in bone, also. Cobalt-60, however, is deposited in the total body, but during decay radiates a very high energy and penetrating gamma-rays.

There are non-radioactive environmental hazards associated with nuclear explosions and the by-products of such detonations also. The heat of the detonation may mobilize heavy metals which will then be free to enter any available ground waters and degrade their quality. Carbon dioxide, produced when a detonation is in limestone formation, could enter ground waters if available which then could become acidic. In turn, disruption of paths of natural ground water movement may result in the contamination of potable waters from pre-existing non-potable aquifers.

Localized water pollution has been associated with several nuclear tests in the past.

There are reports where the heat of the detonation itself has mobilized heavy metals, making them free to enter groundwater, however, this effect can be predicted. The same is true of carbon dioxide, produced when detonation is in limestone, turns groundwater acidic.

To a minor extent heavy sedimentation of underground aquifers and surface waters occurs during the initial and subsequent shocks of the blast.

Natural passages in the rock are significantly altered, changing the seepage pathways of existing groundwaters. This is an asset, however, in the development of water resources.

Hydraulic overpressure can cause "water geysers" where existing cracks in the rocks extend to or near the land surface or where new cracks temporarily open and reclose in the vicinity of ground zero. Sustained pore-pressure increases result in increases of water-table elevation and spring and stream discharges as far as 10-20 kilometers. Vertical acceleration of the land surface near the shot can cause "mud geysers", such as those that resulted from the Long Shot Event, in puddles, small ponds and streams.

Localized general water level declines in the immediate vicinity of the shot are caused by chimney filling.

Some lakes are affected by a temporary increase in groundwater discharge. Others may be tilted and drained by deformation of the land surface, also causing a significant increase in streamflow. New lakes can be formed by vertical displacement along intersection faults.

Surface waters can carry quantities of loose material into the streams and lakes over a long period, creating heavy silting of reservoirs and natural lakes. However, the effect can be avoided.

Such environmental effects could conceivably create general ecological and biological problems. Disturbances of soil and water cause sensitive species of plants and animals to die, disturbing the balance of nature. Spawning grounds of some fish are destroyed by seismic disturbances or sedimentation, for instance. Vegetation with shallow roots becomes easily lost when soil is cracked or broken.

Attached to this briefing paper are five items of information including (A) potential applications, (B) history, (C) scientific and engineering properties of nuclear explosion, (D) potential schedule of underground explosions, and (E) experience to date.

ATTACHMENT A

Potential Applications

A number of potential underground applications of nuclear energy have been proposed. Those considered technically and economically promising are being evaluated. For the following applications some degree of evaluation or planning has been accomplished. In some instances, experimental shots have already taken place and additional shots are scheduled.

CANAL CONSTRUCTION

Several large civil works projects have been suggested. The most notable of these is the construction of a sea-level canal across Central America. The Atlantic-Pacific Interoceanic Canal Commission, created in 1964, is scheduled to report to the President in December 1970.

Construction of a canal across Central America could involve an estimated 168 explosives in the range of 200 kilotons to 4 megatons each, with a total yield of 120 megatons; the explosives used would be of a minimum fission design.

Should a canal be constructed with nuclear explosives, there would be venting of radioactive gases and fine particulates which would be transported through the atmosphere following local weather patterns. Surfance distribution of the shorter-lived products would require relocation of inhabitants to avoid a temporary hazard to public health and welfare in the vicinity of the detonation; however, long-term potential hazards from surface distribution would be minimal.

The potential hazard to public health and welfare arises from the long-term radioactivity initially deposited in the vicinity of the explosive device. This material could migrate long distances through the environment and eventually become incorporated in man's food-cycle and the global inventory of radionuclides.

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MINING

The use of nuclear explosives in the mining and mineral field may have the greatest potential for economic development. Cratering shots have been suggested for removal of the overburden in open pit mining operations. Contained explosions have been suggested for the stimulation of oil and gas fields and for cracking ore deposits for later leaching processing.

A non-AEC estimate of the potential applications in this field includes:

Use	Number of shots	Shot yield (KT)	Total yield (MT)
Gasfield development	100	200	20
Copper leaching	30-50	50	1.5-2.5
Oil shale field development	100-300	50	5-15
Gas reservoir construction	5-25	50	.25-1.25
Total	235-475		27-39

The potential hazards associated with excavations for mineral extraction rest mainly in the processing stages and in consumer utilization. This is due to the presence of long-lived radioactive products mainly krypton-85 and tritium. The krypton-85 is a non-reactive gas and consequently will co-exist with any gaseous products unless it is effectively removed before distribution.

The presence of tritium results in chemical processes in which the tritium atom replaces the hydrogen atom. This happens in any water compounds as well as hydrocarbon compounds such as oil products and natural or derived gas. Because these tritiated compounds behave chemically the same as the naturally occurring materials, it is difficult to remove all the radioactive products before distribution. Some will be passed on into the ambient atmosphere creating the potential of low-level radiation hazards.

HYDROPOWER PROJECTS

Applications in the field of hydropower projects include the diversion of existing water channels and the construction of dams utilizing the lips from crater explosions and the generated rubble for construction material.

WATER RESOURCE PROJECTS

The use of nuclear explosives has been suggested to divert water to water-poor areas, to construct catch basins, and to create underground excavations for water storage and recharging. For these applications the porosity and permeability of the crater or rubble material are important, and the potential for leaching radioacidity into ground waters is increased.

Nuclear explosives have been suggested for a large-scale diversion of water from the Columbia River to the Colorado River. Nuclear explosives have been suggested where such an aqueduct would pass through large undeveloped areas, particularly in Nevada.

HARBOR CONSTRUCTION

Cratering shots have been considered for constructing harbors throughout the world. Detailed plans, Project Chariot, were developed in 1960 to construct a harbor on the northwest coast of Alaska.

GENERAL TRANSPORTATION CONSTRUCTION

Nuclear explosives have been considered for cutting passes through mountain barriers for railroad and road beds and for removing natural barriers in navigable channels. In 1963 the Carryall project detailed the feasibility of excavating a cut through the Bristol Mountains in California, between Barstow and Needles for the realignment of a railroad and a super-highway. Interest in the project was curtailed in 1966 because of lack of progress in developing the evacuation technology and the approaching deadline of the 1972 Interstate Highway program. For railroad and highway excavations, one of the pertinent engineering properties is the foundation stability of the fall back material.

ATTACHMENT B

History

The Atomic Energy Act of 1946 recognized the potential of developing peaceful nuclear energy applications such as power production and ship propulsion. Major progress has been made in both these programs, with the ship propulsion efforts centered on naval vessels.

Development of thermonuclear or fusion explosives improved the potential for other peaceful applications because of the accompanying reduction in radioactive waste products for each unit of energy released. Also, fusion fuels are more abundant and cheaper than fission fuels. Among those other uses are underground application for construction, mining, etc.

The possibility of harbor and canal contruction was first demonstrated in the course of nuclear weapons testing in the Marshall Islands in the 1950's. During this period a number of possibilities were suggested, including a proposal in 1956 to excavate an alternate sea-level canal across Israel to by-pass the Suez Canal. However, an alternate sea-level canal across the American Isthmus received the greatest attention.

In 1957, the Atomic Energy Commission formally established the Plowshare program to explore the whole range of potential engineering uses of nuclear explosives. At that time the Atomic Energy Commission had little experience with underground explosions at depths which produce craters of maximum dimensions or at greater depths where the explosion is confined.

The first contained experiment was RANIER in September 1957. The first Plowshare experiment was Gnome in 1961. The experimental program was halted during the nuclear moratorium period from October 1958 to September 1961.

With the signing of the Limited Nuclear Test Ban Treaty in 1963 constraints were imposed on the Plowshare program as well as weapons testing.

For applications where the energy source is fully contained, compliance with this treaty is assured unless unanticipated releases occur. Applications involving excavation, however, release radioactivity to the atmosphere even though improvements have been made in explosive design and emplacement techniques to reduce the amounts released.

The 1963 treaty does not prohibit cratering explosions if they do not "cause radioactive debris to be present outside the territorial limits" of the country where they are conducted but the radioactive debris must be controlled within a country's territorial limits. The problems associated with excavations in other countries or large civil works projects near national borders are as yet unresolved.

ATTACHMENT C

Scientific and Engineering Properties of Nuclear Explosion

A nuclear reaction lasts less than one one-millionth of a second. The immediate effect of the explosive energy is to vaporize the device and the adjacent media into a spherical bubble of gas and to create a strong spherical shock wave.

The front of the shock wave moves radially outward successively producing vaporization, melting, crushing, cracking, and plastic deformation of the surrounding geologic media. The final physical configuration resulting from a nuclear explosion depends upon yield of the explosive (amount of energy released), depth of burial and the physical and geological properties of the surrounding media. The resulting configurations are cavities, rubble chimneys, rubble mounts, and craters.

Another configuration, important for civil construction applications is the linear crater formed by the simultaneous detonation of a row of nuclear explosive. For this application, in addition to energy yield and depth of burial, the important parameter is the spacing between the explosive charges.

In the case of contained, deep underground explosions, the explosive energy is dissipated in the ground except for approximately 1 to 5 per cent which contributes to an acoustic wave in the atmosphere. The radioactive materials are designed to be contained in the ground except for intentional extraction of selected products.

For cratering explosions, a large fraction of the energy generated is employed to physically relocate the rock and earth above the explosion. The shock and blast waves displace the overburden upward. A large fraction falls back in place and the remainder is deposited around the perimeter of the resultant

ATTACHMENT D

Potential Schedule of Underground Explosions

Available references estimate less than 5 shots per year for research purposees for the next two to five years. Thereafter, ten to thirty shots per year are anticipated for commercial purposes, increasing to 100 or more shots per year, not including those for the canal excavation.

These estimates are only for applications currently to be considered. New applications could add significantly to these estimates.

Research applications in the immediate future under the Plowshare program include:

Name	Use	Date	Tentative Yield (KT)	Approximate Depth (ft.)
Dragon Tail	Gas stimulation	1969	20	3,000
Wasp	do		50	12,000
Wagon Wheel	do		50	12,000
Sloop	In-situ mineral leaching		20	1,200
Ketch	Gas storage		24	3,300
Bronco	Shale oil extraction		40	

ATTACHMENT E

Experience to Date

A number of experimental tests have been conducted that have application to the Plowshare program. These experimental tests can be categorized as contained, in which the explosive effect is confined to regions below the earth surface and the soil is not dislocated, and cratering, in which some amount of the surface soil is relocated.

The cratering tests are designed to develop basic information applicable to future excavations with emphasis on canal excavation. Information is being obtained on the effects of explosive yield and soil composition on crater size. The contained experiments are tailored to specific potential use such as gas development.

A summary of experimental tests conducted under the Plowshare and other programs which have yielded significant engineering information on cratering and containment is contained in Table 1. Additional underground tests of military weapon systems have yielded engineering information, also, but are not listed.

TABLE 1.—EXPERIMENTAL TESTS ON THE UNDERGROUND USES OF NUCLEAR EXPLOSIVES UNDER THE PLOWSHARE PROGRAM

Name	Date	Yield (KT)	Depth (ft.)
Cratering experiments:			
Jangle S [1]	1951	1.2	3.5
Jangle U [1]	1951	1.2	17.0
Teapot ESS [1]	1955	1.2	67.0
Danny Boy [1]	1962	.42	110.0
Sedan	1962	100	635.0
Sulky	1964	.1	90.0
Palanquin	1965	4.3	280.0
Cabriolet	1968	2.6	170.75
Buggy	1968	[2] 1.1	135.0
Schooner	1968	35	355.0
Contained experiments:			
Gnome	1961	5	1,200.0
Handcar	1964	12	1,300.0
Hardhat [1]	1962	4.5	950.0
Shoal	1963	12	1,200.0
Salmon [1]	1964	5	2,700.0
Gasbuggy	1967	26	4,200.0
Rulison	1969	40	8,430.0

[1] Non-Plowshare experiments contributing to an understanding of the technology.
[2] 5 in row.

Senator MUSKIE. I will now ask the sponsor of this legislation, Senator Gravel, to make an opening statement.

Senator GRAVEL. Thank you, Mr. Chairman.

I think my views are broadly known in this area. I would just like to associate myself with the remarks of your statement for the record.

Senator MUSKIE. Thank you, Senator Gravel. We have two distinguished witnesses this morning. First, Dr. Marvin Kalkstein, professor, State University of New York, Ithaca, N.Y.

STATEMENT OF DR. MARVIN KALKSTEIN, PROFESSOR, STATE UNIVERSITY OF NEW YORK, STONY BROOK, N.Y.

Senator MUSKIE. May I express my appreciation to you, sir, for being willing to visit with us this morning and to give us your testimony.

Mr. KALKSTEIN. Thank you. I would like to express my appreciation to the committee for holding these hearings and affording me the opportunity to testify.

I submitted prepared testimony and rather than read it, I would like to just make a few brief comments and then would be happy to discuss the subject that we have on the table today.

Senator MUSKIE. I have not had an opportunity to read your statement so I hope you won't overlook any significant points.

Mr. KALKSTEIN. All right. Fine.

Senator MUSKIE. I think also for the benefit of those in the room that I ought to refer to Professor Kalkstein's record.

He has been associated with Harvard University's Center for International Affairs, Air Force Cambridge Research Laboratories, dealing with atmospheric radioactivity studies, the University of California Radiation Laboratories dealing with nuclear chemistry.

He has served as Chairman of the Federation of American Scientists and at Stockholm International Peace Research Institute involved in a study of international arrangements and control for the peaceful applications of nuclear explosives.

I think also might be included, if it is available, Professor Kalkstein, the study by Dr. Edward Martell to which you referred.

Mr. KALKSTEIN. I would suggest that it be included.

(The following article, by E. A. Martell, is excerpted from "Environment," April 1969 issue:)

By E. A. Martell

Plowing a Nuclear Furrow

IN 1966, AN ARAB-ISRAELI war closed the Suez Canal. At the University of California Livermore Laboratory in Berkeley, an informal group met to discuss a suggestion made by the Laboratory's Director, Harold Brown, to dig a new canal across the Sinai Peninsula from the Mediterranean to the Gulf of Aqaba. Livermore Laboratory is where America's nuclear weapons are developed, and Dr. Brown proposed using large nuclear explosives to dig the canal. After brief study the suggestion was dropped, but the basic idea — using nuclear explosives for earth-moving projects — gradually grew.

The result of that idea is Project Plowshare, the Atomic Energy Commission's program to make peaceful use of nuclear explosives — A-bombs and H-bombs. Turning these giant weapons to peaceful uses is difficult, however, and so far nothing more than experimentation has been possible. Since 1956, there have been a number of excavation projects proposed, but so far all have been abandoned for one reason or another. The digging of a harbor in Alaska, Project Chariot (see ENVIRONMENT, June, 1961), was the most actively pursued of these, but was eventually allowed to drop because of strenuous protests over the radiation hazards that would have been involved. The recent proposal for a nuclear-bomb-excavated harbor in Australia (see "Six Questions for Australians" in this issue) is similar to Project Chariot. There have also been attempts to use underground nuclear explosions to free tightly locked natural gas reserves.

But the most ambitious project in which Plowshare's hopes have been most heavily invested, and toward which much of its nuclear testing is being directed, is the digging of a sea-level canal through Panama.

The Panama Canal opened in 1914, the greatest engineering achievement of its day. It laid a line across 50 miles of formless, inhospitable jungle and connected two oceans which had not been joined in a geologic age. The symbol of success in the battle against disease and nature's intractability, the locks of the Panama Canal (110 feet wide and 1,000 feet long) lifted the most massive ships up and over the continental divide. Dams and whole lakes were needed simply to fill and refill the locks.

Two generations later, at least 500 of the ships now sailing are too large to pass through the Canal fully laden, and two dozen of the United States's aircraft carriers are more than twice the width of the locks. The ships too large to pass through the Canal, excepting aircraft carriers, are mainly the giant tankers which ferry oil across the oceans. These mammoth tankers, many of which can carry five hundred thousand tons of oil or more, have created some imposing problems, including devastating oil-spills.

The difficulty with the Canal, of course, is its locks. Step-fashion, filled one after the other, the locks lift a ship from the Atlantic to the level of an inland lake, to another lake, and then down again in step-fashion to the Pacific. Once built, the system of locks is not at all easy to enlarge, and the 110-by-1,000 feet dimensions which once seemed ample are now cramped and overcrowded.

The greater speed and reliability of modern shipping have reduced the penalty for more roundabout routes for the largest tankers, and the bulk of remaining shipping can still be accommodated by the Canal. However, as the volume of shipping grows, the inducements for a better canal grow with it, and at some point the cost of a more comfortable route between the oceans may become justified. What will the cost be?

There have been three possibilities discussed: enlarge and improve the present canal; using conventional techniques dig a new canal directly connecting the two oceans at sea-level, thus obviating the need for locks; or blast such a sea-level canal with nuclear explosives.

A sea-level canal has some attractions. It would do without the locks, dams, power plants, and other complex and expensive operating requirements of a lock canal. It would accommodate more traffic, would allow more rapid transit of ships, and would be nearly impossible to sabotage.

A sea-level canal also has some drawbacks. Tides on the Pacific side of the Panamanian isthmus run up to twenty feet, ten times the height of Atlantic tides. Strong currents would flow through any direct channel between the two oceans, making problems for navigation. More significantly, a sea-level canal would carry very large numbers of plants and animals directly from one ocean to an-

An experiment conducted at the Atomic Energy Commission's Nevada Test Site on March 12, 1968, produced this ditch about 900 feet long, 300 feet wide, and 80 feet deep, resulting from the simultaneous underground detonation of five small nuclear explosives. The ditch is three times as long as a football field and more than twice as wide. An eight-story building would stand upright in it. The experiment, called Project Buggy, was a step toward the excavation of a sea-level Panama Canal with nuclear explosives.

other; the oceans now have no salt-water communication nearer than the Antarctic. The consequences of such a vast geologic and ecologic experiment can right now only be guessed and not foreseen. Violent disruption of the local ecology — perhaps affecting the fisheries of the Pacific Coast and the Gulf of Mexico — and the extinction of many species are among the possible consequences.

These problems are inherent in any sea-level canal. Several possible routes for such a canal are shown in the map. The numbers refer to the report of the Governor of the Panama Canal Zone written in 1947, in which 30 different alternate routes were proposed. Eight of the routes were considered suitable for sea-level construction; the remainder would have required locks. Only five of the eight are still under consideration. The most suitable site of all for an improved canal, the Governor felt (perhaps not surprisingly), was the site of the present canal. The report, *Isthmian Canal Studies — 1947*, concluded that:

> A sea-level canal constitutes the only means of meeting adequately the future needs of interoceanic commerce and national defense, and such a canal can be obtained most effectively and economically by converting the present Panama Canal to sea level.

The Governor's report was neglected for some time, in part because the digging of a sea-level trench between the oceans was a task which put even the construction of the first canal into the shade, and which would certainly cost billions of dollars. Project Plowshare put some new life into the proposal, however. This program of the United States Atomic Energy Commission, as its name vividly implies, is dedicated to turning the implements of modern war—nuclear weapons—to peaceful uses.

TNT is an explosive and can be used in a number of constructive ways, particularly in blasting away earth and stone to make way for roads, tunnels, bridges, dams, and canals. In the sense that H-bombs are also explosives, they too might be used for workaday tasks. A single H-bomb could substitute for millions of tons of TNT.

The difficulty, of course, is that one normally does not use millions of tons of TNT at one time—or at all. H-bombs can be made smaller, and are, but their expense rises very fast in smaller sizes, diminishing their advantage over TNT.

"Large" and "small" take on different meanings when nuclear explosives are discussed. Small nuclear explosives are usually said to be in the kiloton range; that is, they are equivalent to some thousands of tons of TNT. Large nuclear explosives are in the megaton range, equivalent to millions of tons of TNT. A better standard of comparison is probably the earthquake. A megaton-range nuclear explosive set off underground is comparable to a damaging earthquake. The famous San Francisco earthquake was very roughly equivalent to a 100-megaton explosion. A small nuclear underground explosion is roughly comparable to a small earthquake of the kind that Californias have become accustomed to.

The proposal has been made that groups of large nuclear explosives be set off in a chain across Panama, blasting a trench for the oceans to flow into. The proposal is now being studied by the Atlantic-Pacific Interoceanic Canal Study Commission which is weighing it against the other alternatives for improving the passage between the oceans.

The Canal Study Commission was established under Public Law 88-609 signed by President Johnson on September 22, 1964. Chaired by Robert B. Anderson, the Commission was given the broad responsibilities of determining the need for a sea-level canal, evaluating the merits of various possible canal sites, and assessing the feasibility and costs of canal construction by conventional and nuclear excavation methods. The target date for completion of this study was extended recently from June 30, 1968, to December 1, 1970. In its Fourth Annual Report, July, 1968, the Study Commission stated:

> The Commission has not yet reached any final conclusions, but it appears highly probable that additional isthmian canal capacity will soon be needed and that a sea-level canal will be found technically feasible. Much remains to be accomplished in the investigation of engineering and navigation problems. Current construction cost estimates recognize the possibility of large contingency costs, particularly in nuclear excavation at the present stage of development of the technology. Timely execution of the remainder of the Atomic Energy Commission's planned program of nuclear excavation experiments is essential to develop more reliable cost estimates and to demonstrate the technical feasibility of nuclear canal excavation.
>
> Although the Commission's studies in all areas of its investigation are incomplete, the advantages of a sea-level canal in comparison with a lock canal continue to appear impressive economically, militarily, and politically.

E. A. Martell is a senior scientist on the staff of the National Center for Atmospheric Research and a member of the Colorado Committee for Environmental Information. A graduate of West Point, he received his Ph.D. in nuclear chemistry from the University of Chicago in 1950. As a staff member of the Armed Forces Special Weapons Project from 1950 to 1954, he was engaged in studies of the effects of nuclear weapons during the first nuclear cratering tests in Nevada and in the 1954 CASTLE thermonuclear tests in the Pacific. He has written extensively on the subject of fallout and particularly on the problem of radioactive iodine released by underground tests (including "Underground Tests and Radioactive Iodine" in the December, 1963 Environment).

Nuclear Blasting Problems

In conventional excavation, chemical explosives are used to shatter rock, which is then removed by machinery or human labor. Nuclear explosives eliminate step two: the explosion itself is so violent that earth and rock are blasted away. The entire process is illustrated on pages 6 and 7. A deep hole is drilled well beneath what will eventually be the bottom of the crater, and a nuclear explosive is lowered into it. Rows of nuclear explosives would be set off simultaneously in the nuclear canal construction plan. The explosion releases a good deal of radioactivity, of which most is trapped in the crater or is deposited near the site. A fraction of the radioactive material, principally the gases, may travel much farther.

If the explosives have been buried at the proper depth, the final result will be a shallow ditch with a heap of ejected material on both sides. The rock for some distance underground in both directions will be highly fractured and crushed. At greater distances the rock will simply have been pushed out of position and may slowly readjust itself after the explosion.

As can be seen from the illustration, the lip of ejected matter that surrounds the crater is roughly as high as the crater is deep. For the deepest excavations this might mean a heap of rubble a quarter mile high, spread for miles in both directions. This great mass will lie exactly as it falls on the shattered sides of the crater. Most of the questions regarding the feasibility of nuclear excavation of a sea-level canal regard the stability of this structure, which would be undercut by strong currents flowing through the canal, washed by the heavy rains of the area, saturated by groundwater, and shaken by nearby explosions and perhaps by earthquakes triggered by those explosions.

Our experience with nuclear excavation is limited, to say the least, and is non-existent in the soil and climate that a sea-level canal would pass through. A number of very small (as these things go) nuclear explosives have been tested at the United States's Nevada Test Site, all of them in the dry rock or soil of the desert. All but two were less than four kilotons. The most recent, in December, 1968, was 35 kilotons and resulted in some of the highest fallout levels recorded in many cities of the West and Northwest since atmospheric testing was abandoned (ENVIRONMENT, December, 1968).

The results of this testing have not been entirely consistent. Two early small cratering experiments were named SULKY and PALANQUIN. (All peaceful nuclear explosions, for some reason, are given the names of conveyances.) SULKY was an 0.1-kiloton explosion which, out of deference to the recently signed Test Ban Treaty, was buried at a depth (90 feet) considered sufficient to guarantee that all the overlying earth would fall back into the crater, trapping the radiation beneath it. This test went as planned, and a few months later, in April, 1965, PALANQUIN was conducted. The same calculations were used to decide the depth at which this test was buried (380 feet), but in this case the explosion, a four-kiloton blast, burst out through the hole which had been drilled to emplace the explosive, creating a volcano-like crater and releasing most of its radioactivity.

On the basis of these and subsequent experiments (five since 1963), formulas have been derived for determining the right depth for implanting an explosive of a given size to achieve the largest possible crater and for predicting the size of the crater which will result. These calculations can now be made easily for small explosions.

There is some question of whether the calculations will apply to very much larger blasts, however. The most favored sea-level canal route, Route 17, would run across 57 miles of Panama, cutting through hills more than one thousand feet above sea level. A single group of four explosives totaling twenty-five megatons would be combined with many smaller groups of explosives for a total of 166 megatons, according to present calculations.

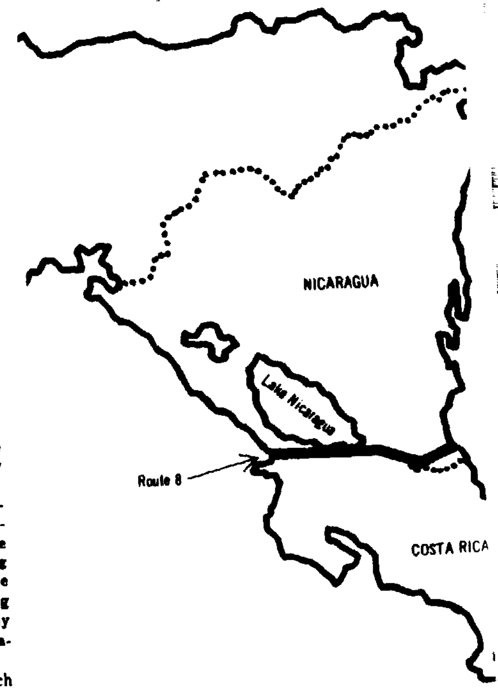
By comparison, the largest cratering test which has so far been conducted is 100 kilotons. A sea-level canal would require at least one explosion 250 times as large.

By the simplest calculations an explosion produces a crater whose volume is proportional to the size of the explosive. As the size of the explosive increases, the volume of the hole it makes increases by the same amount. The diameter of the hole, however, increases much more slowly. For very large holes, for instance, only a slight increase in width means a very large increase in volume.

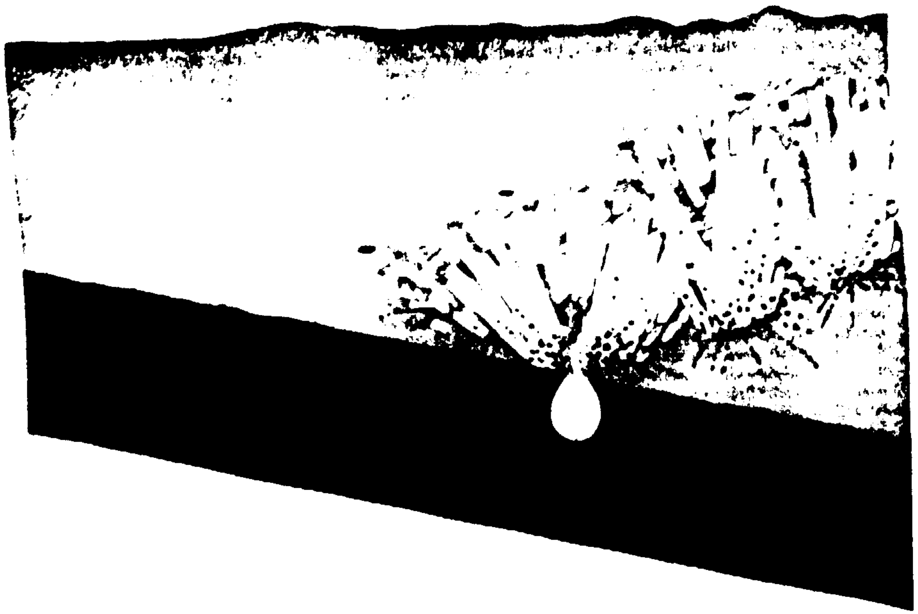
But it is width and not volume that interests the canal builder, and to get a ditch twice as wide, he must use an explosive between eight and sixteen times as large. In practical situations, it is difficult to predict in advance just exactly how large an explosive should be to give a particular size hole. Only experimentation can give the answer for a particular range of explosive sizes in particular soil or rock conditions.

Present calculations of the explosives needed for a sea-level canal assume that experience with relatively small explosives at the Nevada Test Site will be directly applicable. These estimates are almost certainly unrealistic. Very large explosives in the megaton range are likely to be less efficient earth-movers, unit-for-unit, than smaller ones, and present estimates may be off by a factor of five. If so, the cut through the continental divide on Route 17 would require three 25-megaton and one 50-megaton explosives set off together — an explosion of 125 megatons. If such massive yields are required, the difficulties of nuclear canal construction, perhaps already insurmountable, would be magnified greatly.

These uncertainties make it very difficult to guess at the cost and even the feasibility of nuclear



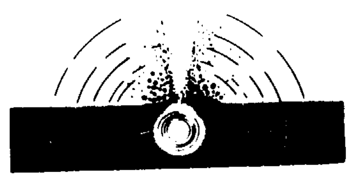
Five possible routes for a sea-level passage between the Atlantic and the Pacific. Route 14 is the Panama Canal; Route 10, next to it, is as short but would not interfere with operation of the present canal. Routes 8, 17, and 25 are under consideration for excavation with nuclear explosives.

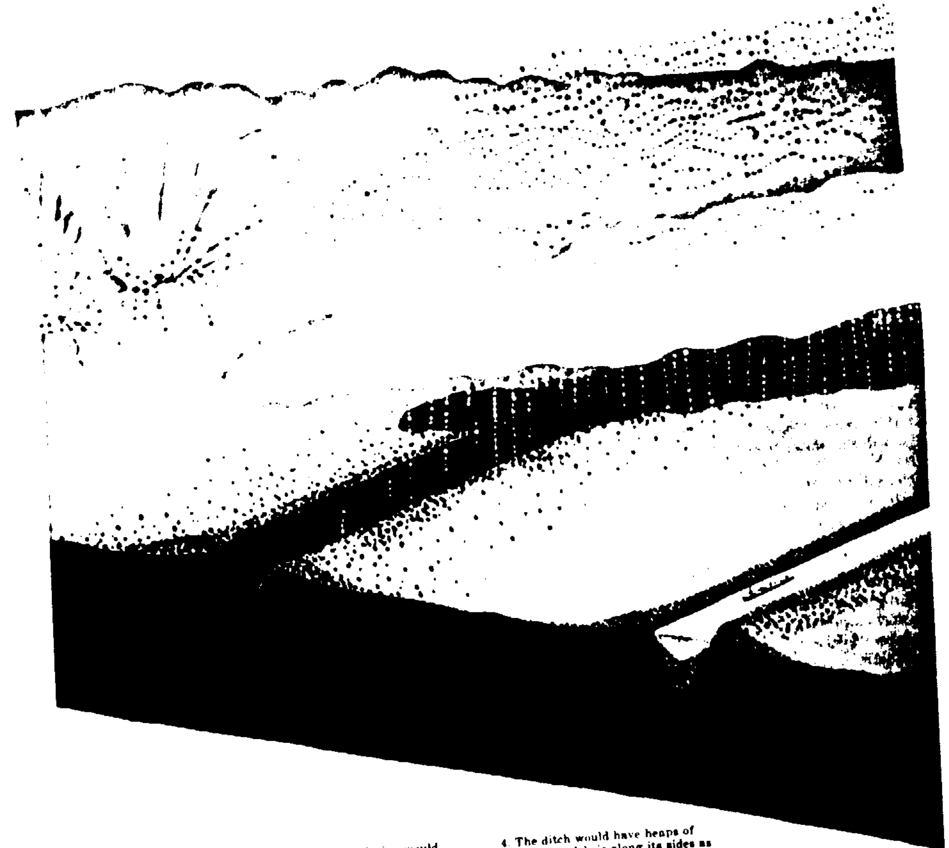


To dig a ditch across Panama with nuclear explosives.

1. Drill a row of holes and then lower H-bombs into them. The depth of the holes and the sizes of the bombs should be carefully calculated to produce a ditch of the desired width. Past experience on which to base the calculations would be limited and derived from far smaller explosions.

2. Set the bombs off several at a time. The largest group might create an explosion with more than the energy of the San Francisco earthquake of 1906. The shock might trigger earthquakes elsewhere in seismically active Panama, and it might shake down the sides of parts of the canal already completed.





3. Much of the earth blasted loose by the explosion would
fall back into the ditch, partly filling it, or onto
its sides, creating a "lip" of ejected matter. Highly
radioactive smaller particles would drift a few miles before
settling; this would be the local fallout. Radioactive gases
produced by the explosion, including some of the more
hazardous materials (iodine 131, strontium 90, cesium 137)
would rise to great heights and might travel hundreds
or thousands of miles before being brought back
to earth by rainfall.

4. The ditch would have heaps of
radioactive debris along its sides as
high as it would be deep. The bottom
would be filled with radioactive rubble,
and the rock would be shattered for considerable
distances in both directions. Heavy rainfall, strong
tidal currents flowing through the canal, and the shocks
of earthquakes and other explosions might crumble the sides
of the canal, requiring expensive conventional excavation.

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excavation. They also make it difficult to estimate the danger of damage from radiation and other side effects of the nuclear explosion.

Radiation Hazards

When a nuclear cratering explosion takes place, the explosive and a large mass of surrounding earth are completely vaporized. The expanding vapors and gases push up through the ground surface into the atmosphere. Much of the radioactivity produced is trapped, however, by earth falling back into the crater. Radioactivity attached to smaller particles of the pulverized rock and soil materials falls more slowly and is deposited as local fallout near the crater. The radioactive gases and their radioactive decay products remain in a radioactive cloud and drift downwind. This radioactive cloud contains large percentages of the radioactive materials of greatest biological significance to man, including strontium 90, cesium 137, and iodine 131. It is the prediction of the subsequent fallout distribution of the radioactive debris from this cloud that poses the most serious question for the radiological safety of nuclear excavation projects. The local fallout also is a special problem in that it would impede and complicate all construction operations in the immediate area and might contaminate soils to levels which would prevent their use for agricultural purposes for some time to come. In the case of nuclear sea-level canal construction, some of the longer-lived radioactive products trapped in the crater would be leached out and redistributed in the local environment by the action of rain, groundwater, crumbling of the crater's edge, and the action of tidal currents.

Based on the experience from the SEDAN and DANNY BOY Plowshare shots, the fraction of the total radioactivity which appears as local fallout has been estimated to be four to ten percent for optimum nuclear cratering depths.

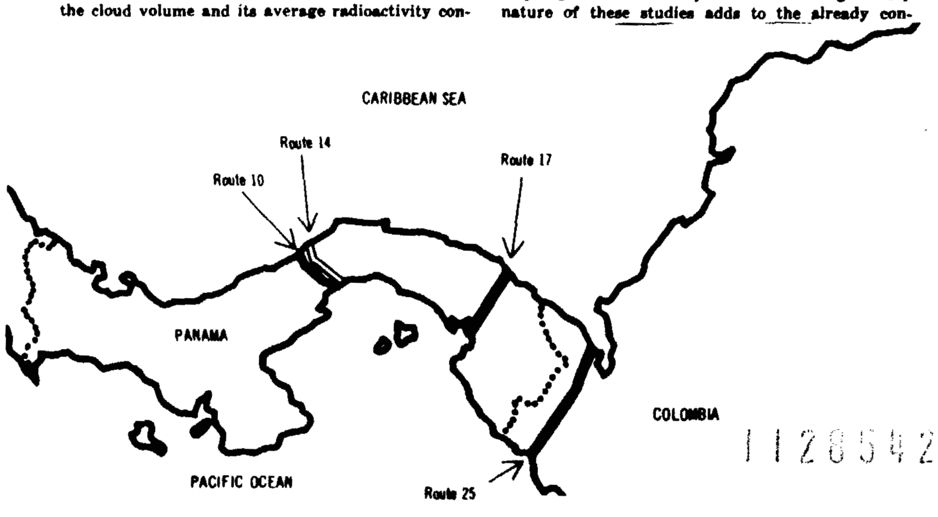
Information on the composition of debris in radioactive clouds from nuclear cratering shots was reported by Bonner and Miskel (1965). They show the extremely high degree of enrichment of strontium 90, strontium 89, and cesium 137 in the escaping cloud from the DANNY BOY nuclear excavation experiment. Based on very limited information about the cloud volume and its average radioactivity concentration, Bonner and Miskel estimated that ten to twenty percent of the more volatile products was released. For hazard evaluation purposes it must be assumed, in the absence of better information, that as much as 40 to 80 percent of these radioactive products may have escaped. The extent of escape of iodine 131, tritium, and other volatile fission products and neutron-induced radioisotopes also may be high.

This substantial atmospheric contamination with the radioactive isotopes of greatest biological significance to man by nuclear cratering shots poses an especially serious hazard for the nuclear canal project. Because the heights of debris clouds from high-yield nuclear cratering shots in a complex medium are very uncertain, both the transport of the cloud debris in the variable wind systems described below and the resulting radioactive fallout areas would be highly unpredictable.

Weather Conditions

Meteorological studies related to the feasibility and safety of nuclear canal construction along Route 17 in eastern Panama and along Route 25 in northwest Colombia are being carried out by the Environmental Science Services Administration (ESSA). Plans were made to carry out two years of weather data collection on Routes 17 and 25 to answer two important questions: What would be the size and orientation of the area around each proposed nuclear excavation canal route which would have to be evacuated because of local fallout? What would be the frequency of occurrence of winds and weather conditions which would restrict radiation hazards to evacuated areas?

Four manned meteorological stations, one near each end of these two routes, were established. Stations along Route 17 were installed in July and August, 1966. Stations on Route 25 were set up in July, 1967. Because of budget limitations, meteorological data collection on Route 17 was terminated on December 31, 1967, after only eighteen months of operation (instead of the planned two years). The meteorological station at Loma Teguerre on Route 25 was closed on February 29, 1968, after only eight months of operation. The fragmentary nature of these studies adds to the already considerable difficulties in predicting the fate of fallout from the canal excavation.



Recently ESSA issued a report presenting weather observations on Route 17 for the period July, 1966, to June, 1967. Mean temperatures were near 80° F throughout the year with relative humidities ranging from 75 to 100 percent. Annual rainfall was high, marked by a dry first quarter and frequent precipitation throughout the remainder of the year. For the one-year record available, the twelve-month total rainfall at one station was 68.34 inches, and at another the eleven-month total was 102.06 inches of rain; there were no data for the heavy rainfall month of July. Except during the dry season, the number of days on which it rained exceeded twenty days per month. Much heavier precipitation was experienced in some areas along Route 25. At Andagoya and Quibdo in the upper Atrato River Valley in the province of Choco, the registered annual rainfall was 268 inches and 433 inches respectively.

If the possibility of heavy fallout due to rainout of radioactive cloud debris is to be minimized, it would be necessary to schedule nuclear excavation shots during the dry season. At Soskatupu on Route 17 the dry period is limited to the first quarter of the year. At Pidiaque at the other end of the Route, it is relatively dry from December through April. Wind regimes are similar at both locations throughout the dry season, with moderate easterlies predominating between 5,000 and 30,000 feet altitude, and strong westerlies predominating between 30,000 and 55,000 feet. Winds below 5,000 feet are predominantly from the north at Soskatupu and from the northeast at Pidiaque. The frequency of northwesterly surface winds is highest at Soskatupu, the eastern-most end of the Canal.

This complex pattern of winds, coupled with the fact that the altitude distribution of radioactive clouds from nuclear row-cratering shots is highly unpredictable, provides a dismal outlook for the prediction of fallout areas. Radioactive debris from the base surge and from clouds below 5,000 feet would be carried to the south and southeast across southeastern Panama and western Colombia, or to the southwest along the canal route. Debris in clouds above 5,000 and 30,000 feet altitude would be carried westward and eastward respectively, creating an extended east-west radioactive cloud source for which the fallout pattern would be widespread and unpredictable. Areas which may receive significant fallout include Costa Rica, Panama, northern Colombia, and northwest Venezuela.

Apart from the main meteorological problem of predicting radioactive fallout transport, the frequent rains and fog and the hot, humid tropical climate would impede all engineering operations, conventional and nuclear alike.

Other Problems

In addition to difficulties discussed above, other direct and indirect effects of nuclear explosions raise further doubts about the use of such excavation methods. Locally there would be massive devastation by throw-out and air blast. Nuclear excavation would result in a thick ejecta lip of rock and mud over a zone some ten to twenty times the width of a canal constructed by conventional means. The direct air-blast wave generated by the nuclear cratering shots could cause heavy damage to forests, structures, and equipment to distances of tens of miles. The effects of the shock waves traveling through the ground would be highly unpredictable, partly due to variations in geologic structure and partly because of our lack of experience with very high-yield explosions. The fact that Central America is an active earthquake area further magnifies the difficulty of predicting effects. There is a real possibility that the nuclear detonations would trigger large earthquakes at great distances. Direct damaging effects on structures could be expected out to distances of 60 miles or more. For the large nuclear explosions involved, unexpected effects could occur. A major slope failure in a previously excavated section of the canal is one of the possible consequences. Damage that might be caused by nuclear explosion generated water waves in coastal areas is another possibility that would be difficult to evaluate in advance. In addition, air pressure waves generated by the explosions could be reflected from the upper atmosphere and give rise to damaging effects in developed areas at distances up to several hundred miles. Gross errors in predicted effects at large distances should be expected.

One of the more important political and moral issues involved in nuclear canal construction is the necessity for large-scale evacuation of local populations. Minimal safety measures would require the evacuation of land areas within 25 miles in the upwind and crosswind directions and over much larger distances downwind in the path of radioactive fallout. Preliminary evacuation plans anticipate that over 30,000 people from areas near Routes 17 and 25 would have to be resettled elsewhere for a few years. Apparently the AEC would be responsible for radiological surveillance of these evacuated areas and the air over them, and would also have to be prepared to assist in the emergency evacuation of other areas which accidentally received significant levels of fallout. The people to be moved would include frontier settlers and primitive Indians. Evacuation plans on Route 17 would affect (1) the Cuna Indians near the Caribbean end of Route 17, (2) the Choco Indians in central and southern regions, (3) Colombian immigrants in the west and in scattered coastal areas, and (4) colonists from crowded coastal areas of central and western Panama who have resettled in the areas near Route 17 during the last decade. Studies of these population groups and their cultures, food sources, and agriculture are in progress for the purpose of evaluating radiological safety and resettlement problems. The displacement of these several cultural groups in large numbers for a period of years is not simply a matter of costs and logistics. The extent of the injustice to the evacuees can be seriously magnified if the level of radioactive contamination and physical damage to their lands and food supplies prolong the period of resettlement and necessitate changes in their way of life.

Construction of a nuclear sea-level canal along Route 17 would all but destroy the Cuna Indians and their culture. They number a few thousand and live along the Darien coast, on islands, and along rivers in the nearby jungle areas as they have for centuries. The Cuna are independent people who subsist on fish and bananas, and trade in coconuts. Because of their brutal treatment at the hands of the Spanish, the Cuna Indians became hostile to all outsiders. They would resist attempts to evacuate them, and, undoubtedly, many would hide in the impenetrable jungle nearby. Those not found and forceably evacuated would be killed or injured by the nuclear explosions. Finally, even after the

canal's completion, many of the evacuated Cuna would not be resettled in their former homelands. Large areas of their lands not contaminated or otherwise disrupted by nuclear operations would be taken over by new communities and permanent installations necessary to operate and defend the new canal. What price tag is to be placed on the sacrifice of one of the last surviving primitive human cultures of Central America?

Another serious question is the possibility that connecting the two oceans by a sea-level canal would result in an ecological catastrophe. While this question relates to the desirability of constructing a sea-level canal per se, the only alternative would be a lock canal constructed by conventional methods. Ecologists have discussed the possible biological consequences of mixing the two oceans with varying degrees of concern. Large-scale extinction of marine animal species is possible due to changes in water currents and water temperatures and to invasion by competitive species. Many facets of this complex and vital conservation issue are discussed by Briggs who predicts the possible extinction of thousands of species of marine fauna in the Eastern Pacific coastal areas and states:

> The important question is: Should the sea-level canal project be undertaken at all? What is the value of a unique species—of thousands of unique species? Currently, many countries are expending considerable effort and funds in order to save a relatively few endangered species. The public should be aware that international negotiations now being carried on from a purely economic viewpoint are likely to have such serious biological consequences. Does our generation have a responsibility to posterity in this matter?
>
> A biological catastrophe of this scope is bound to have international repercussions. The tropical waters of the Eastern Pacific extend from the Gulf of Guayaquil to the Gulf of California. Included are the coasts of Ecuador, Colombia, Panama, Costa Rica, Nicaragua, Honduras, El Salvador, Guatemala, and Mexico. While the prospect of such an enormous loss of unique species is something that the entire world should be aware of, these countries are the ones that will be directly affected since their shore faunas will probably be radically changed.

The need for a sea-level canal has not been clearly established. It does have the distinction that it is the only type of canal that can be built using nuclear excavation methods. Its advantages would be economy of operation and more rapid transit of larger numbers of ships. However, an improved lock canal might satisfy projected traffic demands for many years to come. A sea-level canal would be less vulnerable to sabotage than a lock canal. On the other hand, construction of a lock canal by conventional methods would be feasible, safe, and free of the hazards and other problems of nuclear construction. It also is noted that a lock canal, whether sea-level or not, would be free of the navigational hazards that would be ever present for a nuclear sea-level canal, including strong and variable tidal currents, channel obstructions, and incipient slides due to slope failures.

Costs and Alternatives

One factor which may tip the balance among the various canal alternatives is cost. Nuclear excavation techniques appear to be less expensive when initial excavation costs are considered. Simple comparison of nuclear versus conventional construction costs on the same long, remote canal route therefore appear to favor nuclear methods by a large factor. However, a fair comparison requires that all important contingencies and alternatives also be taken into account.

The proposed method of construction and a preliminary cost estimate for each canal route are given as follows in the Canal Commission's most recent report:

PROPOSED SEA-LEVEL CANAL ROUTES

Route	Length	Construction Method	Estimated Cost (millions)
8. Nicaragua—Costa Rica	140	nuclear	1,900
10. West of Canal Zone	46	conventional	2,500
14. Panama Canal	46	conventional	2,300
17. Panama (Sasardí-Morti)	57	nuclear (166 megatons)	747
25. Colombia (Atrato-Truandó)	100	nuclear (271 megatons)	1,050

Because of many uncertainties, actual costs might be appreciably higher than the tabulated estimates which include only engineering and excavation costs. For instance, the most recent estimate for nuclear excavation along Route 8 is only 1.6 billion for construction. However, the evacuation of very large numbers of inhabitants of both Costa Rica and Nicaragua would be necessary and would raise total costs to about five billion dollars. Due to its great length and the high population density in the vicinity, it is unlikely that Route 8 will be seriously considered.

Nuclear excavation is much cheaper per unit volume excavated, partly because the excavated material is explosively ejected from the crater and does not have to be hauled away. However, as we have seen, the resulting massive ejecta lip or the fractured sides of the canal themselves, or both, may slide back into the canal. Essential crater lip drainage and removal, conventional excavation or construction to prevent slope failure, channel dredging to remove slide debris, and other required remedial construction, would all add to the cost of nuclear excavation.

Route 17, which apparently would be cheapest, has begun to look less favorable since the discovery of very weak, wet clay shales over twenty miles (about one-third) of the canal route, in the Chucunaque Valley. Only very gently sloping walls of the canal would be stable in such weak material, but nuclear explosions tend to produce fairly steep-sided craters. Whether this difficulty can be overcome will be decided only after tests of very large explosives in similar material. Should conventional methods be required in Chucunaque Valley, construction costs for Route 17 would be comparable to those for Routes 10 and 14. Considering the uncertainties involved, it does not seem possible at present to distinguish among the various sea-level canal routes as to cost.

The original Panama Canal cost three times as much as had been originally expected. In view of the unfamiliarity with nuclear construction methods, it does not seem unreasonable to expect a similar escalation in the actual cost of a nuclear sea-level canal.

Cost comparisons have not included a number of major items of expense for a nuclear canal at a remote site. Not included are treaty costs covering such items as site acquisition and rights-of-way, indemnification against damages and contamination consequences of nuclear detonations, and annuity payments to the host country. Estimates also do not include the costs of essential new permanent facilities in the nuclear canal site area, including defense installations, permanent road networks and air facilities, as well as naval and port facilities beyond the minimum required for canal operations.

Conversion of the existing Panama Canal to a sea-level canal or construction of a new sea-level canal by conventional methods along Route 10 would involve few of these additional costs. Conversion of the present Panama Canal to a highly improved lock canal would cost only about one billion dollars—substantially less than any realistic estimate of total cost for the proposed nuclear sea-level canal. The improvement would involve equalizing the level of the two Panama Canal lakes, eliminating the intervening locks, and adding large new locks and other modifications in order to handle larger ships and much more traffic.

Also, there are a number of alternatives to a nuclear canal outside Panama. One interesting possibility has been proposed recently by the Hudson Institute: damming up the lower Atrato and San Juan Rivers to form two large lakes and subsequently connecting them by a canal and joining them to both oceans by locks. The estimated cost is several hundred million dollars. Hydroelectric power from the dams might pay for the entire project, leaving the canal as a kind of bonus. (The same report suggests that there is no longer a significant tie between U.S. national security and the Panama Canal.)

In brief, when all the very considerable uncertainties are taken into account, there seems to be no significant cost difference among the various alternative approaches to a sea-level passage between the oceans. Improving the present canal or perhaps constructing a new lock-canal through Colombia would be cheaper than blasting a sea-level canal with nuclear explosives. The use of nuclear explosives, unlike other proposals, would involve still unpredictable risks of earthquake and of local and world-wide radioactive contamination as well as greater dislocation of local populations.

Testing Planned

Before the feasibility of nuclear excavation of a canal can be finally determined, additional large-scale cratering experiments are obviously essential. Recent correspondence between Robert B. Anderson, Chairman of the Canal Study Commission, and Glenn T. Seaborg, Chairman of the Atomic Energy Commission, describes the high-yield nuclear cratering experiments which are contemplated.

Mr. Anderson, on August 23, 1968, wrote:

> *Dear Mr. Chairman:*
>
> At our August 16 Commission meeting we carefully reviewed the progress of our investigation to date and came to the conclusion that we should again ask the Atomic Energy Commission to press for timely execution of the remaining experiments needed to determine the technical feasibility of nuclear canal excavation.
>
> We have twice asked the Congress for extension of our study time, and our final reporting date is now December 1, 1970. The additional study time was granted by the Congress with the understanding that it should be adequate to permit the completion of the AEC's supporting nuclear excavation experiments. We will be extremely reluctant to request additional time, and it is our present intention to render a report on the prescribed date with or without a final determination of the feasibility of nuclear excavation. The coming need for additional canal capacity and the necessity for the United States to make adjustments in its existing treaty relations with Panama will not permit indefinite postponement of decision on future canal policy.
>
> During the August 16 meeting we were pleased to have Mr. John S. Kelly meet with us and describe the status and plans for the *Plowshare* nuclear excavation program. He summarized the planned nuclear cratering experimental program as follows:
>
> FISCAL YEAR 1969
>
> *Schooner* — a 40-kiloton point charge cratering experiment in hard rock.
>
> FISCAL YEAR 1970
>
> *Yawl* — several-100-kiloton yield point charge cratering experiments in hard rock.
>
> *Galley* — a row charge experiment in varying terrain involving simultaneous detonation of about 7 nuclear explosives each in the yield range of a few kilotons to a few hundreds of kilotons.
>
> *Gondola* — a cratering experiment planned to be conducted in a wet, weak, clay shale whose design will depend on the characteristics of the site to be selected.
>
> FISCAL YEAR 1971
>
> *Phaeton* — a one-megaton point charge cratering experiment in hard rock.
>
> Mr. Kelly advised that this program will be difficult to carry out because [of] the additional workload, the tight schedule, and the problems of obtaining the necessary support and approvals. We recognize that this represents a large increase in effort and costs in Fiscal Year 1970 and appreciate the problems such increases pose. However, our Engineering Commissioners and the Engineering Agent, after reviewing the program described by Mr. Kelly, believe it to be the minimum on which we can make specific recommendations concerning the use of nuclear explosives to excavate a sea-level canal.
>
> We fully recognize and appreciate the effort you have made and continue to make to carry out nuclear cratering experiments in support of our investigation. We are aware of the many obstacles to the timely execution of the additional experiments as described above. Nevertheless, we urge you to seek whatever funds are required and early approvals for the above cratering experiments. If there is any way in which we can assist, please let us know.

Dr. Seaborg replied:

> *Dear Mr. Anderson:*
>
> Thank you for your letter of August 23, 1968. My fellow Commissioners and I appreciate having the Canal Study Commission's up-to-date views on the need for data from our nuclear excavation experiments for your use in considering the technical feasibility of nuclear excavation of a sea-level canal. We are also familiar with the assurances provided to and views of Congress in connection with the extension of time for your study.
>
> We are very conscious of the problems created for the Canal Study Commission by our previous inability to obtain requisite approval for our nuclear cratering experiments. Now that this problem seems to have been alleviated for appropriately designed experiments, you may be assured that we will do everything possible to carry out the additional required experiments.
>
> As you mentioned, we will shortly be considering certain of the required additional experiments in connection with our Fiscal Year 1970 budget. In this connection, also, it is our intention to do everything possible, within the resources made available to us, to carry out the program outlined to you by Mr. Kelly.

The AEC is being pressed to complete this minimum test program before December 1, 1970, the final reporting date for the Canal Study Commission. Funds for Project Yawl have been included in the Administration's 1970 fiscal year budget (which begins June 1, 1969) now before Congress. There is little doubt that conduct of the proposed cratering tests would be contrary to our obligations under the Nuclear Test Ban Treaty (see pages 10 and 11). If these high-yield cratering tests were carried out at a continental site, they also would constitute a threat to the public health and safety.

If these four nuclear cratering experiments were carried out, we would obtain improved knowledge of crater scaling and other effects of high-yield

excavation shots. However, it is doubtful that the results would be adequate to establish the feasibility of nuclear canal construction. The experience with PALANQUIN demonstrated that nuclear cratering in a complex medium can be highly unpredictable. The numerous possible conditions and mechanisms which can lead to a slope failure for nuclear craters in a wet environment remain with us. And if, as expected, larger explosions prove to be proportionately less effective than the small tests so far conducted, the increased explosive size requirements would magnify the problem of crater slope stability as well as all nuclear explosion effects and hazards.

The safety and the economic, political and ecological feasibility of a nuclear-dug canal all remain open questions. With or without the experience of the additional tests, nuclear construction may be ruled out by the Canal Study Commission for any one or a combination of many reasons. It is the Commission's announced policy "to avoid any public statements which tend to favor one method of canal construction over any other until such time as its ultimate recommendations are made public," so it would be quite possible for dangerous and expensive high-yield nuclear cratering tests to be done although they would no longer have a function in terms of the canal. There are serious questions, therefore, about both the possible ultimate nuclear construction of a canal and the benefits and risks of the cratering tests themselves. □

Senator MUSKIE. You may proceed, Professor Kalkstein.

Mr. KALKSTEIN. I realize that the subject of nuclear testing and, particularly now, underground nuclear testing and the safety aspects of it has been a particularly vexing one for Government, for the AEC, I am sure, but I would suggest most of all for the American public. Past experience, dating back almost since the inception of the nuclear era and the beginning of nuclear testing, has afforded little opportunity to the public to understand what is happening in the area of nuclear testing both with regard to the purposes of the tests and with regard to the effects.

The hazards of nuclear testing became known to the public after the 1954 test. I think the opening of the subject to public concern and awareness was not done initially or principally by Government agencies. I think it was done by private individuals such as Dr. Ralph Lapp who took the pains to study what had happened and to publicize the results. The Limited Test Ban Treaty that we arrived at in 1963 I think was largely the result of the concern on the part of the public about the hazards particularly from the fallout. I think the feeling was on the part of many, and particularly in the public, that having achieved a limited test ban, they could stop worrying about the effects of nuclear testing.

In fact, the experts who were doing research and working in the area of nuclear effects recognized even before the test ban that these hazards would not necessarily completely disappear when we went to underground testing. As early as 1962 Dr. Edward Martell pointed out that there were hazards of release of debris from underground testing. In a research paper, he suggested that high levels of iodine 131 and other debris that were found in the southeastern United States, after the Soviet Union had resumed atmospheric testing and after we had begun our underground testing program, could well have been attributed to underground tests. I don't think that his article or the arguments against it resolved in any definitive way the source of that radioactive debris. I think the thing that was significant was that he was attempting to bring to public attention the potential dangers even from underground testing. I suggest attempting to bring to public attention since at the time I was associated with Dr. Martell at the Air Force Cambridge Research Laboratories, and I think there was a sense that there were attempts on the part of Government agencies to try to keep this quiet and to make it difficult to bring this to light.

I bring this up since I think one of the concerns being expressed by the introduction of this legislation is that the Government agencies charged with the responsibility of safeguarding the public interests aren't necessarily always as diligent as they should be in that pursuit.

STERNGLASS REPORT

To bring an example of a more recent case of concern about the effects of radioactive fallout, I would cite the concern expressed by the findings of Dr. Ernest Sternglass. I am not supporting the findings or suggesting that he is right or wrong in his findings. But I feel that Government, and particularly AEC, has been rather derelict in the way in which they have tried to deal with the findings of Dr. Sternglass.

I think he raises a serious question. He may be wrong, but it is one where the public has a right to be informed, to be concerned, and one where particularly I think Government has a responsibility to respond with all its powers involving as much of its resources as necessary to try to ascertain whether he was right or wrong.

Instead, it seems the response has been one to rather lightly dismiss what he has suggested and not to attempt to really allow for a full debate on the subject and hopefully for support of work which would allow us to deal with the question from a more knowledgeable position.

Senator RANDOLPH. Mr. Chairman, may I interrupt at this point?

Senator MUSKIE. Senator Randolph.

Senator RANDOLPH. Professor, you have spoken of a specific instance where you haven't used specific language. The Atomic Energy Commission has failed to act with candor and concern in reference to these findings. Is that correct?

Mr. KALKSTEIN. Yes, sir.

Senator RANDOLPH. Can you give us other instances where this seems to be true?

Mr. KALKSTEIN. I think the whole subject was treated, I believe, rather gingerly and at least not involving full candor at the very beginning of the testing program until Dr. Lapp made it a public issue.

It is hard to detail specifics then. I think the case specifically which I just mentioned, Dr. Sternglass', is one. I think the concern back in 1962 about underground venting was another case. Other instances of the AEC's seeming disregard for public concern include the responses to hearings in Nevada on the seismic effects of testing and to public hearings on the siting of nuclear reactors in Vermont and elsewhere.

CABRIOLET TEST

Another example which would suggest to me ambivalence of concern was the initial timing and postponement of the Cabriolet test where initially this had been scheduled to occur in late winter or early spring of 1967. I believe John Finney in the New York Times, January 29, 1967, had an article in which he indicated it was being postponed for a month or two because of concern about the fact that it might otherwise have occurred at a time when the cows were just coming out to pasture, when iodine 131 that might have been released in that test then had a chance of contaminating the pasturelands and creating a potential hazard.

I think in a sense the AEC at that time was being aware and perhaps being conscious of the hazards of nuclear testing, but at the same time in their policy and in their program of nuclear testing, even underground, they had up to that point acted in seeming disregard of such factors.

The other question of concern for the public interest—and I think it will probably be necessary for the committee to go beyond just the specifics of hazards and pollution—is that that particular postponement that I mentioned made it so that the new date of the shot coincided precisely with the reopening of disarmament talks in Geneva at a time when we were more or less settled on draft of the Non-Proliferation Treaty. Thus, we were planning to proceed with an

event which, if anything, would have pointed the finger rather strongly at the United States as acting in seeming disregard of the interests that we were proposing to other nations that they have in trying to hold down the nuclear weapons program.

Senator RANDOLPH. Mr. Chairman, I appreciate your permitting me this time because it may be that I will have to be in and out of this hearing. I do know that you, as chairman of this subcommittee, are aware that we do have deep concern for what I call involvement of the public interest.

Therefore, I think it is very, very important that we not isolate single cases; that we take them, combine them, and then see what the case is in reference to the public interest.

So you are very helpful, Professor, in exploring with us in this hearing your feeling about public interest, candor and concern being, to a degree, lacking in the Commission's effort.

Mr. KALKSTEIN. Thank you, Senator.

Senator MUSKIE. Thank you very much, Senator Randolph.

I understand Senators Randolph, Gravel and Baker all have other committee sessions to attend.

I would hope that they would feel free to interrupt so that they may put any questions on their minds even before your testimony is completed, Dr. Kalkstein, because I think it is an important subject in which every Senator is concerned.

Senator Baker?

Senator BAKER. Mr. Chairman, thank you very much, and I apologize to you and the witness for having to go to a conference committee. I am going to have to leave shortly.

Do I understand that you are speaking now of release of radioactive iodine into the atmosphere as a potential or some aspect of underground nuclear testing that would cause iodine-131 to be dangerous even though it didn't enter the atmosphere?

VENTING

Mr. KALKSTEIN. No. I am speaking specifically with regard to the venting into the atmosphere, the concern as it was in the case of venting even before we had the Test Ban Treaty.

The concern with the Cabriolet test was that iodine-131, along with other products, could vent into the atmosphere, could get into pasture, into milk, and as such was a potential health hazard.

Senator BAKER. I wasn't sure of that portion of your testimony and I do understand that you are speaking of the danger of venting of radioactive substances.

Senator GRAVEL. Most of the testimony that I have heard from the AEC and the others states that whenever radiation occurs it is in such small quantities that it is diluted. Therefore, there is no danger.

The question I pose and have yet to receive an answer to, is who is keeping track of all the radiation that does go into the atmosphere, and if somebody is keeping track, do we have a total inventory? If continued testing in the manner projected, and with all of the power reactors that are being built, which accelerate the pollution of our environment, at what point will it become a danger? Or does this dilution enable it to go on without limits?

Mr. KALKSTEIN. I think you are raising questions to which there are some answers and of which further questioning and further study is required.

ENVIRONMENTAL MONITORING

My understanding is that for the underground testing there is monitoring of the immediate test site. I am not sure just how far out that goes. I suspect that the AEC has its own health physicists involved. I would also guess that the Public Health Service has some responsibility for that monitoring. In addition, Public Health Service does have a network—I am not sure how active it is now, but it was quite active in the past when there was atmospheric testing—a network of stations throughout the country. Other countries, Canada, Sweden, do have monitoring stations that routinely monitor the amount of radioactivity in the air and put out, I guess, their monthly compilations of what the radiation levels are. These tend in these compilations to give an averaging or smearing picture.

I don't think that is any real attempt to look for anomalous situations that may occur subsequent to any particular test after the debris has been monitored out to whatever limit the AEC monitoring of the test site may have, even though the chances of a serious anomalous event from a small-sized test might be quite small. I would worry that with large events such as would eventually be conducted in the excavation program when one gets to using megaton weapons in a cratering event where it is expected that there is venting, that one must avoid what happened with the first large thermonuclear use.

UTAH SHEEP KILL

It was a very anomalous event with regard to the Japanese fishing vessel where a certain amount of fallout came down. I think the incident even with the sheep in Utah, the testing of nerve gas, was certainly an anomalous event, one that could not have been predicted but one that took place in the absence of public scrutiny and the kind of review that I think this legislation is aiming for.

Senator GRAVEL. If I could just pursue the point, I interpret your answer to mean that they are measuring.

CONTAMINATION INVENTORY

The inference I was leading up to was a total inventory of the environment. If we are making one unit of effort economically at present, or one unit of contamination and we throw 10 times more reactors on the line, we make a substantial economic investment, then probably later on we will be accelerating the rate of contamination by 10 to 50 times at present.

So my question is at what point will the environment become saturated and how can we tell this if there is no broad inventory being taken?

Mr. KALKSTEIN. I think I would agree with your concern. I would say my impression is we are far from doing the kind of job that you are suggesting needs to be done, and I even think when you put it initially in terms of economic units that that may be part of the problem

with leaving the control and the monitoring and the responsibility for safeguarding the public health in the hands of the same agency that has the responsibility or at least sees its mission as making these various nuclear programs economically feasible.

As a matter of fact, in the testimony presented before the Joint Committee on May 8th of this year, when they were dealing with commercial activities under Plowshare, H.R. 477 was the legislation they were considering—a representative of the Department of Interior proposed an amendment which would have given the Department of Interior responsibility for safeguarding the public interest in terms of contamination of resources and environment.

The response by one of the committee members was that what you are suggesting might add time to the program, it might add cost, and the purpose as they saw it of the program was to make it economically feasible. So I think even the question of regarding economic units is important.

Senator MUSKIE. Senator Randolph?

Senator RANDOLPH. I have one final question, Professor Kalkstein. You have spoken of the Utah incident concerning sheep.

Mr. KALKSTEIN. Yes, sir.

REVIEW

Senator RANDOLPH. From the standpoint on nuclear tests that there must be a planning when a test is programed, as to the safety of the plan. Is that correct?

Mr. KALKSTEIN. Yes, sir.

Senator RANDOLPH. Are the Under Secretaries of the National Security Council the group that does the review of the test and approve it insofar as safety is concerned? Do the Under Secretaries have that power vested in them?

Mr. KALKSTEIN. I am not sure. Again, reading the testimony from the May 8th hearing I just referred to, Chairman Holifield of that committee rather vigorously defended the position that it was AEC's responsibility.

As a matter of fact, one of the proposals was that, when we get to commercial uses of nuclear detonations, that the contractor and the AEC have responsibility for safety, and he said that under this legislation it is the AEC's responsibility. I believe that the final say in this area, at least the decision and the consideration of the issue in arriving at that decision is by the AEC.

In the exchange that took place between the committee and the representative of the Department of Interior it was pointed out that, indeed, the AEC does go to many agencies for advice, but it is purely advice. These other agencies do not seem to have any real say in the decision that is being made.

Senator RANDOLPH. Mr. Chairman, when the AEC gives the green light, does that have to have the approval of the National Security Council?

Mr. KALKSTEIN. I am afraid I can't answer that.

Senator MUSKIE. The staff answer to that is "Yes."

Senator RANDOLPH. Well now, Professor, we will assume the answer is "Yes," and I feel sure that we are informed correctly.

Is this the appropriate body to make this review and approve the environmental effects which we are thinking of as we open these subcommittee hearings today?

Mr. KALKSTEIN. I would say with regard to environmental aspects, with regard to particular peaceful nuclear applications, whether it is by nuclear detonations of nuclear explosives or by the use of nuclear reactor, I would say that body would not be the appropriate one.

Again in the same testimony I just referred to, in the transcript it is indicated that in all tests for peaceful application purposes the principle of public safety has been applied by the AEC or by whoever made the decision.

It was also indicated in that testimony that this principle has not always been adhered to in the case of weapons testing because in weapons testing the primary concern of Government, I guess, is that they have a program which is vitally connected with national security in terms of the military aspects of that question.

Senator RANDOLPH. I was talking about civilian, of course.

Mr. KALKSTEIN. All right. In that case, I would certainly suggest that the National Security Council should not be the agency to be dealing with the civilian applications.

Senator RANDOLPH. I think I agree with you, sir, and I have given some thought and study to the subject.

Thank you, Mr. Chairman.

Senator MUSKIE. The staff points out, and I think it might be so, that the National Security Council is involved only when they are explosive devices.

It is not involved, for example, in connection with nuclear powerplant radiation hazards but only explosive devices.

Mr. KALKSTEIN. Right.

Senator MUSKIE. We interrupted your opening remarks, Professor Kalkstein, I wonder if you would continue with those.

PUBLIC POLICY CONSIDERATIONS

Mr. KALKSTEIN. So far I have not dealt much with technical aspects of the hazards question. I would like to get into it. I would like, though, to preface my remarks about the technical aspects by saying that I at least feel it it quite important that we deal not only with the technical aspects but with the political considerations.

I realize that the committee and the Senate is much more experienced in that area than I am, but I found in dealing with these issues such as the one that you are considering today, that although the technical information is available and most of the debate, particularly among the scientists, seems to be in terms of the technical issues, the decisions go far beyond that.

They involve the economic, social, and political implications and I would say from my own view, these considerations tend in the final judgment, generally to outweigh the technical factor.

I would like to say one other thing. We have seen on most issues, such as in the initial debate, before we had the test ban, about fallout and hazards of fallout, and most recently in the debate on ABM, that you have scientists who disagree. I think it is important for Congress

and the public to understand a little bit how scientists work, particularly in dealing with these issues. The fact that they disagree does not or should not suggest that the facts are different or that they are operating with different facts. The same basic facts are available for all.

However, science involves doing something with facts. Any scientist—and here the scientist is no less fallible than anyone else—any scientist, in coming to a scientific judgment on facts, forms these by operating with a set of assumptions with which he then assesses the facts and then comes to a conclusion. So I would suggest that it is quite important when we look at technical issues that we not solely look at the facts. The thing that really quite often has to be questioned if there are disagreements is to look at the assumptions. When the scientist is operating within the laboratory or his professional journals he is quite careful to list very specifically all the assumptions because he knows that the other scientists when operating professionally will look almost at the first instance at those assumptions. I think it is quite important here also that we not just limit our considerations to the facts but be aware that assumptions are involved, estimates are involved, and this is a valid way of dealing with the subject.

I would like very briefly to just enumerate the concerns here. I believe that the committee will have further testimony that will go much more specifically into the issues of the technical hazards.

SEISMIC EFFECTS

The concerns that one has with underground testing are in a number of different areas. One area is the seismic effect. We had considerable concern before the Amchitka explosion as to whether or not there might be sizable earthquakes following that explosion, whether or not there might be tsunamis, tidal waves, which would be generated as a result of the explosion.

The result seems to be that there was nothing of any real serious nature. Again we can look at what happened and see the difficulties that scientists or that anybody that might be concerned with this issue faces. Within the preceding 24 hours of that earthquake, there was apparently a major earthquake in Santa Rosa, Calif. Had that earthquake occurred anywhere in the following 24 hours, conclusions about that event might have been very difficult. This suggests that one has to look very carefully at the consequences and the results. Apparently, it would have been a misconnection to have attributed the Santa Rosa earthquake, say, to the Amchitka test, no matter when it took place.

One reason I bring this up is that so far the attempt to deal with this issue of review of nuclear testing has been to concentrate on the hazards that are involved without, as we talked about earlier and as I intend to talk about later on if there is an opportunity, concern for the broader aspects of the issue.

In a sense, experience says that the AEC and Government agencies are quite careful in preparing for these tests, the fact that they then conclude a successful test, if anything increases the difficulty in trying to then assess and to review future tests, and that this just adds to the weight of the position that the tests ought to proceed. One could well

see us going along with another 200 tests, each one of which may be a success which then allows less argument for each succeeding test until somewhere along the line a catastrophe occurs.

Of course, then is when the review will take place. I think what you are trying to do now is quite proper. That we ought to be reviewing these aspects.

In the seismic area, there are no definite answers. We can't say, "Yes, there will be an earthquake, there will be serious hazards." We can't be sure or know that there won't. This is what makes it difficult because each time there wasn't, it looks like it was a pretty sure thing, which isn't so.

The question was raised by Kenneth Pitzer back in the spring, as chairman of an ad hoc committee on safety of underground nuclear testing, that the whole issue of seismic safety was a serious enough one and one on which we don't yet have answers that I believe he suggested refraining from large underground tests until that be resolved.

The next area——

Senator MUSKIE. May I ask a question on that?

Mr. KALKSTEIN. Yes, sir.

Senator MUSKIE. Is it possible to get answers on the question of seismic safety? In other words, what additional precautions can be taken to avoid any risks whatsoever? In connection with Amchitka explosion, what precautions might have been taken that were not taken in advance of that explosion?

Mr. KALKSTEIN. I am not sure. I would expect that the AEC took every precaution that they could have conceived of at that time. I wouldn't suggest in the actual testing program that there is any attempt to be slipshod or to be less than as careful as they possibly can.

I think it is not a question just of what precautions or what added precautions. I think the question that needs real study by a commission such as is proposed here is that one review all the information that is available and I think probably the basic question isn't what precautions or what are the maximum precautions, but whether or not our state of knowledge suggests that we can in fact proceed with these tests.

I think a more realistic approach may have to be not how do we make them 99.9 percent safe, but do we in fact have enough assurance that we can and should proceed with these tests and, there again, it requires that the committee get beyond just the technical question of the hazards, but it requires that there be someone who can evaluate overall the question of test policy. What are the purposes? How important are they? And what is the conflict then between that and the public interest with regard to the hazards?

Senator MUSKIE. In your judgment should the Amchitka explosion have taken place?

Mr. KALKSTEIN. I think enough question had been raised prior to it. I think the position of Dr. Pitzer should have been taken into consideration and I think the Amchitka explosion should have been suspended, at least until the kind of review as this legislation suggests had taken place.

Senator MUSKIE. Is there any agency which can now give the kind of review that this legislation proposes and which in your judgment is necessary?

Mr. KALKSTEIN. I am not sure how well the Government is presently set up to deal with these questions. I imagine there are probably people in the Coast and Geodetic Survey that could have a hand in making this sort of review. One of the difficulties one gets into is even a question of who the agency is, how the expertise is developed. I suspect most of the expertise in the seismic area has been developed by experts connected with the nuclear weapons or explosives testing program and therefore pretty much tied to the program in one way or the other. How much, outside expertise there exists at the present is something I am not sure of.

AEC SELF POLICING

Senator MUSKIE. In any case, you are not satisfied that whether or not it is equipped to do so, the Atomic Energy Commission does give adequate consideration in this regard?

Mr. KALKSTEIN. Since I feel that the last test should not have taken place, I therefore must conclude that it wasn't adequate consideration.

Senator MUSKIE. I think something that you said prior to the question by the Senators suggests that you feel that in an agency like the Atomic Energy Commission, which becomes involved in its principal mission and succeeds, its growth and development and expansion tend to become inhibited with respect to the study of these kinds of risks.

Mr. KALKSTEIN. I am not sure if they are really inhibitive. I think their view tends to become narrowed, so I would suspect that a study that might be done by the AEC might be more inhibited internally than a study done by some outside less interested party, at least less interested with respect to the results of the test. I wouldn't suggest that the AEC is inhibiting any outside agency from doing the kind of study we talked about except perhaps in that the funds at present go to the AEC and aren't available to outside agencies.

Senator MUSKIE. You think it would be a sound proposition to make the AEC a self-policing agency with respect to these kinds of risks?

Mr. KALKSTEIN. I think the AEC for close to 25 years now has been a self-policing agency. I recall that in the very early sixties there was some discussion and contemplation of a split in AEC functions, taking away the regulatory functions from the operational functions. I guess that discussion was short lived since we still have the AEC operating both in an operational fashion and a regulatory fashion, and still in a position also to make the decisions and the judgments as to whether its operations are in conflict with the public interest in terms of safety.

I don't mean to suggest that the AEC has attempted in any deliberate way to shirk its duties with regard to safety. I believe they are fully conscious of this. But in a sense you know when I talk about science operating by assumptions, what happens is that generally an investigator who is acting for a given enterprise or agency usually brings the assumptions of that agency into play in how he evaluates a given situation.

Senator MUSKIE. Of course this kind of question is involved not only with the AEC but with other agencies. For example, the Corps of Engineers licenses all manner of activities that have environmental

effects—dredging, construction of dams, and so on. This committee, having rather direct jurisdiction over the Corps of Engineers, has felt for a long time that the Corps ought not to be self-policing but ought to be subject to the scrutiny of view of others whose principal concern is environmental affairs.

The same thing is true with the AEC. I think it is true with respect to the Department of Defense, which we made somewhat of a scapegoat in this connection for a long time, and I think it is this concern that leads to the Senate's approval earlier in this session of legislation involving the Environmental Quality Council in the Office of the White House.

One of the critical issues in the shaping of that legislation was the question of whether AEC and Corps of Engineers and other similar agencies should be self-policing with respect to environmental affects. I think what is involved is not only the internal inhibitions that tend to build around an agency whose primary mission is interrelated with environmental effects from different kinds of activity, and I think to the extent that you could address yourself to these points it would be very helpful.

Mr. KALKSTEIN. Fine. I would like very much, and I did to some extent in my prepared statement attempt to address these questions. And one of the things that I indicate in my statement was with regard to the specific issue that we are talking about. I see this independent commission as proposed by this legislation as only a first step to giving a greater sense of public control of the issues involved and particularly environmental aspects.

Before I get off that question I might say I support the formation of the independent commission but really only as a first step in that I even would be a little concerned with the Environmental Quality Council that is proposed.

Now one of the problems again that many of us have seen arise in terms of the Government's attempts, and particularly the executive department's attempts to deal with issues that have a technical component has been that too often it is done by selection of some select committee. Whether intended or not, more often than not the results come out suggesting that the committee by design or otherwise was stacked in a particular way. I think there is great tendency, even if the people are independent and not particularly working for a given agency, to come out with the sort of answer that allows a program to move ahead.

So I think it is quite important at least somewhere along the line that Congress, who I see at least at this point as the representatives of the public in making this a question of public control and of public interest, must come up with some effective mechanism of its own that at least allows it to adequately review given issues, to review the findings of administrative bodies, and to come up with an independent judgment of its own.

Senator MUSKIE. You were on the question of the various hazards that are involved.

Mr. KALKSTEIN. Right. I would like now to get to what I consider the more imminent and potentially more serious hazards. These are the ones that would be caused by venting, by the release of radioactivity.

The experience that we have had with regard to underground nuclear testing was that before the Limited Test Ban Treaty there was considerable amount of venting. There was no treaty obligation which said that radioactivity could not be released into the atmosphere. Tests were conducted underground, I am sure with as much caution as possible, but one had as much as a quarter of the tests conducted before the test ban that had venting.

Since then—well, as of April, about 10 tests have vented. These, I guess, primarily were smaller tests, not necessarily conducted deep underground, but we are getting to the point now particularly with excavation uses of nuclear weapons where the dangers from venting and the question of the hazards and other implications of venting become quite serious.

The last such cratering experiment, Schooner, was one in which abnormally high levels of debris were observed beyond our boundaries. They were observed in Canada. We have an obligation under the limited test ban treaty to prevent debris from passing beyond boundaries. The treaty prohibits any test, even underground, if radioactive debris does get beyond the boundaries of the territory on which it is conducted. We are at the point now I think with regard to the excavation experiments where, in addition to the hazards which I will get back to, we are in direct violation or potentially in direct violation every time we proceed.

TREATY IMPLICATIONS

As a matter of fact, at the hearings in 1963 on the limited test ban treaty held by the Foreign Relations Committee Chairman Seaborg of the AEC, when asked about whether that treaty would inhibit the Plowshare program, suggested that one could proceed with the kind of tests that were necessary for things like excavation for about another 5 or 6 years. That would bring us just about to today. This is 1969. Now he said in his testimony that beyond that it would probably be necessary to change the treaty or get some other agreement that would allow us to proceed.

I am not proposing at this time that particularly this committee worry about what a new treaty ought to look like. I would suggest that even the testimony of Chairman Seaborg and the results that we have had so far from cratering tests and particularly looking at the future schedule of cratering tests which will be larger, would suggest that in terms of meeting our commitments to the treaty we must stop now on those tests.

RADIATION HAZARDS

As far as the radio active hazards, the figures again are rather difficult to ascertain. The AEC, when it monitors venting, monitors just what is present on site, the external radiation gamma doses that are present on site. It doesn't then include in the inventory of what is vented that debris that may have gotten into the atmosphere and carried downwind and away from the test site.

In some cratering experiments one might expect—depending on who makes the estimate—10, 20, 40 or more percent of certain fission products to be vented and to escape into the atmosphere. In addition,

gaseous fission products are more liable to escape and a number of the most serious fission products from the point of view of hazards to man have gaseous or low-volatile precursors, so if there is venting and release of these precursors one may have enrichment in such debris as Strontium-90, Strontium-89, and Cesium-137, all of which have very serious consequences in the biosphere to man.

I think it is very difficult to say and Senator Gravel asked earlier about the point at which these hazards become serious enough or threatening enough. It is obvious the trouble is if we wait until we get to the obvious we may have done a lot of damage along the way.

Many effects of radiation are still not well understood. I think it is to the AEC's credit that over the period of about the last 15 years they have supported a tremendous amount of research in terms of radiation hazards, in terms of biological effects, in terms of radiation standards. During that period we have seen the standards pushed generally to lower and lower limits. We still have indications, as suggested say by Dr. Sternglass, that there are certainly instances in the human development in certain periods where there may be extreme sensitivity such as genetically there is. There may be extreme sensitivity during the fetal period after a woman has conceived a child.

So that it is very difficult to say at this point how serious the low levels of debris that may be released from underground testing or from a cratering experiment may be.

Senator MUSKIE. You have made your point. I take it it is very well documented that the standards have been lowered steadily. Does this suggest that we are too eager to move ahead with activity of this kind without adequate advance and study review and consideration? Obviously, if the earlier standards were too high, didn't we jump too quickly to permit the testing?

Mr. KALKSTEIN. It may be. I would suggest if we go back to about 1956, which at least is when I got into the area of atmospheric radioactivity, there was very little that was known. In a sense what you had—and this sort of gets back to the question of, say, the AEC wearing two hats—you had two programs on different tracks and they were both proceeding, neither one was being held up for the other, so that we had one program which was concerned about the safety aspects and the hazards and which was accumulating information, accumulating knowledge, moving in the direction of greater attention and greater degree of responsiveness with regard to the standards for public safety, but at the same time the other track, the operational track, the one of applications, whether it was nuclear weapons applications at that time, or eventually potential civilian applications, was another track in which that train was also in motion.

I believe what your question suggests and where we are particularly at now with regard to the question of peaceful applications of nuclear explosives, is that at least that train be held in the station until we really have the track well mapped and the guidelines for that journey mapped out.

Although the subject of these hearings isn't to get into the feasibility aspects of these applications with regard to the technical capabilities or the economics, most of the people who have looked at that subject would raise serious questions as to whether or not we were ever going to get to the point where they are going to pay off.

One can raise further the question certainly that whether or not we can move that way, need we be in such a hurry to do it. It doesn't seem to be in the case of applications any question of vital national security interests or anything else which would dictate that one proceed at all permissible speed.

Senator MUSKIE. We seem to do that over and over again. DDT and the pesticides is one recent example of getting the train moving before knowing fully its destination. I think thermal affects from nuclear power plants is another one where we are going on the track without adequate advance study and review of consequences.

The internal combustion engine is a well established example where we were on the track before we knew fully the implications, which, when it comes to the question of radioactivity, are so irrevocable that perhaps the experience of the AEC in constantly lowering permissible limits of radioactivity is enough of a warning sign so that we ought to give real attention at this point in connection with the subject we are studying here. I think you are saying that.

Mr. KALKSTEIN. Yes. I would like to go a little bit further.

I indicated in my statement I think it is quite important for some Government agencies or some legislative bodies to deal with these questions in terms of the broadest implications, such as you mention the internal combustion engine. Generally as in the case of the nuclear program the agencies have a specific mission in mind. Their mandate isn't really to be overly concerned—certainly, the AEC was mandated to have public safety in mind, but most agencies have a specific mission.

DOD is another example where I don't think they can be faulted for proposals, weapons proposals, or any defense budget they come up with in terms of at least discharging their mandate. They are not asked to consider priorities, they are not asked to consider the implications of what they are proposing beyond the objective that they are trying to satisfy. And the AEC is in that position also.

So I think all of these programs, and particularly now that we have been in the area of modern technology with far spread consequences of their application, all these areas require that there be some means by which the broad view, the implications other than specific objectives, have to be considered. Up to now we haven't seen this happen.

In the past year or couple of years there has been concern about the military-industrial complex. I would regard that as a symptom rather than an illness, a symptom of the same sort of thing, the same illness in the sense that we are talking about now.

The United States has become a highly competent technological society. We have developed a can-do attitude after World War II. Give us a problem and we will put industry and technicians to work and we will solve it. What happens is one solves it in the narrow sense in which the problem is defined. If in the coming year when Congress and the Senate considers the budget, suddenly defense isn't the No. 1 priority but the cities are, or the environment is, we should be equally concerned about an urban-industrial complex or environmental-industrial complex.

I think it is quite important that there be provided somewhere as this commission suggests a first step, some means by which the public interest and its broadest views are considered and any program of technology considered in its broad implications.

VENTING

Senator MUSKIE. In discussing this question of venting, what causes venting and to what extent can we control it, to what extent is it unpredictable, in what ways have we had unpredictable effects of this kind develop, and so on?

Mr. KALKSTEIN. OK. There are a number of aspects of the venting, a number of ways in which venting might occur. One of the things one has to look at if one is worried about containment of radioactive debris is where containment takes place and how it takes place.

We have a number of different types of underground detonations in terms of whether or not they are intended to be fully contained. There may be no subsidence of the earth's surface, no cratering, in the event of very deep underground tests or applications such as for gas stimulation and getting oil from shales and things like that where one would expect that the surface of the earth would not in any major way be ruptured.

We have had in some isolated instances a case where one still might get venting because there is an implacement hole that has to be drilled to put in the weapon and we have had at least one instance of where the implacement hole provided a channel for venting. I would concede that this is probably a fairly rare occurrence. We have other events where there is a certain degree of subsidence.

What happens when an underground detonation takes place is that there is tremendous heat and pressure where there is melting of material, and the pressure wave pushes this back so one gets a cavity that will contain a large amount of radioactive debris. One also gets some fracturing, cracking of rock above. Eventually there is cooling and there is a certain amount of rubble that spills into this cavity.

If one has the detonation taking place at such a level, an intermediate level where there is a certain amount of subsidence so that this rubble cracking may reach close to the surface, then one has extra avenues of escape. One has created fissures in the overlying mantel over the cavity which allows debris to escape.

In the case of cratering, where the purpose is to change the surface of the earth, what you actually have in detonation is after the detonation you have material actually being lifted off the surface of the earth, thrown up into the atmosphere and then falling back in such a way that some of it falls back into the crater, some of it falls to form a lip. But here you have obvious rupturing, cracking of the surface with material impelled by the force of the explosion. Therefore, whatever pressure is impelling is a means of pushing forth radioactive debris. You have here a means by which the debris may get out. The fallback material will carry some of it back in, but one has an avenue again to get out.

Another possibility, although perhaps remote, is that—and I don't think we have had such a case—in a sense you are dealing in cratering with the creation of a bubble which sort of hits the top, blows off, and then the material falls down. I suspect this may be a low probability but one might even have a case where occasionally bubbles are punctured at a given point. There may be a possibility of getting a larger channel that we have had before, in which case a large amount of debris

may come out. The experience in the cratering cases is that if the detonation takes place close enough to the surface, that a sizable fraction of the debris has a chance to get out.

TREATY IMPLICATIONS

Senator MUSKIE. You said awhile ago in answer to another question that insofar as the limits imposed by the test ban treaty are concerned, we carry our activity in connection with Plowshare to the outermost limit at this point. What does that mean practically? Does it mean that in order to develop Plowshare into a feasible program of nonmilitary uses the treaty must be modified to reduce the safeguards that the treaty encompasses?

Mr. KALKSTEIN. My comments about having now reached the limits applied specifically to excavation projects, the cratering programs. I would suggest it implies that one needs—whether by treaty amendment or new treaty, or whatever—international agreement to proceed if one wants to go ahead with this program.

I would like to suggest that the U.S. position has been that Plowshare and the benefits to be derived are benefits available to all in economic terms. I believe Dr. Teller in his testimony in 1963 on the test ban suggested that the benefits may even be greater for other nations than it is for the United States. I would suggest at the very least that the wishes of other nations as well as the U.S. wishes to be considered in this regard.

Under the nonproliferation treaty we are asking other nations to abstain from nuclear programs. Therefore, it is quite proper that the major nuclear powers, that the United States and Soviet Union probably be the agencies through which Plowshare programs may be developed, and in fact the development would have to be more or less unilateral or perhaps bilateral. However, the objectives of the program are not unilateral or bilateral objectives. They are international objectives.

So I would even suggest—and here I go beyond just the question of the excavation technology—that although development of Plowshare may be a unilateral function, that the United States should not unilaterally make the decision to proceed with that development.

In other words, the ratification of the objectives ought to be by international agreement. We ought to be willing, if need be, to accept a moratorium on Plowshare until there is evidenced by international agreement that the objectives are desired by the world community. Then I would suspect it would be very easy to get the necessary agreements to proceed with excavation experiments or any other kind we might wish to pursue.

Senator MUSKIE. Would those objectives require revision of present limits on this kind of activity?

Mr. KALKSTEIN. With regard to excavation and cratering one would have to change the treaty. The limit now is quite clear. It says no radioactive debris beyond the borders of the nation conducting the test. The question of no radioactive debris or how you detect it is one which isn't particularly well defined in the treaty. Some spokesmen have suggested that one is concerned about acts that may constitute a public hazard. The treaty wasn't written that way.

In that particular area of expertise, our laboratory was involved in low-level detection of radioactivity in the atmosphere.

I would suggest that once one gets venting, and one even gets gaseous radioactive products from the deep, presumably well-contained nuclear explosions, when radioactivity is present in the area, the residence time for such debris in the atmosphere may be from days to weeks which is enough time for debris to get beyond the boundary.

It may be such by going out with the kind of monitor, say, that the Public Health Service uses, they will not find levels of debris that are present but constitute no public hazard. I would suggest if one were intent enough in terms of monitoring for violation of the treaty, one could probably find factors of tens, hundreds or even thousands greater sensitivity in that area. I would suggest if one spent enough effort to detect the debris outside boundaries, most often in the case of venting one would detect it. It is just that normal means of monitoring aren't set up to serve that purpose so that whether monitored or not I think cratering experiments do represent a definite violation of the treaty.

CRATERING EXPERIMENTS

Senator MUSKIE. Cratering experiments involving apparently the possibility of large explosions to get maximum results and maximum economic benefits?

Mr. KALKSTEIN. Yes, sir. The last test, the Schooner test, was a 35-kiloton test. I believe there is one proposed, the Yawl, which is 100 kilotons to be conducted in 1970. There is one—I forget the name for it—which is to be a series of row charges including some that may be a couple of hundred kilotons. One has been proposed, Phaethon, which would be a 1 megaton.

If one ever got to the actual application such as the Panama Canal, they are talking in terms of using explosives as high as 5 or 10 megatons. The total is a couple of hundred megatons used almost totally for cratering purposes, therefore allowing large releases of venting, and if it turns out that too many of their extrapolations from small to large cratering tests are inaccurate, they may wind up having to use much larger amounts of nuclear explosives than they are presently contemplating now.

So I see in the future we are getting into larger and larger tests with potential for more and more release of radioactivity.

Senator MUSKIE. We are just talking about the test phase now. If Plowshare becomes implemented—and this means that we are going to be dealing not only with large explosions but also with applications around the globe on an increasing scale—what are the implications of that?

Mr. KALKSTEIN. It has a number of implications. One point would be that at present we have been carrying out our testing program in test sites that I am sure have been selected for consideration of public safety in terms of their remoteness, in terms of the media in which the tests are conducted, so that our testing I would presume is being carried out under the most favorable circumstances with regard to safeguarding public safety.

If we get into operational circumstances, except for the proposal of a cut in the mountain pass between California and Nevada, we are

certainly not going to make canals in any of these remote areas, we are talking about carrying out operations in areas where there are populations involved and where there are flora and fauna.

We haven't talked about ecology yet, where there are ecological factors involved, where the consequences, dangers, and immediate hazards would be potentially much greater than what we have had from our testing program. We are talking about doing this throughout the world.

Senator MUSKIE. Aren't we likely to end up with the kind of risk that we attempted to eliminate with the limited test ban treaty?

Mr. KALKSTEIN. This is a concern if we get to doing things like the Panama Canal, and we haven't talked about whether it is a feasible or sensible thing to do, even disregarding the hazards. We are getting into the hundreds of megatons of energy release, which then says we are getting to the potential for radiation release which was attendant upon some of the major test series that we had before.

Senator MUSKIE. Is it conceivable that safeguards could be developed to reduce the venting from that kind of excavation? This is all pretty much open excavation, isn't it, and doesn't it have to be?

CANAL APPLICATION

Mr. KALKSTEIN. It is not open in the sense of detonation taking place above ground. The device is below the surface and in fact the cavity that is formed will be below the surface. But it is the case in the Canal Zone that a number of safeguards will have to be taken.

Ferber and List of the Environmental Sciences Services Administration in a paper they had in "Bioscience" of March this year suggested that an exclusion zone would be necessary for the workers while the work was going on, an exclusion zone of about 5,000 square miles of land area would be necessary in excavating the Panama Canal. In addition, in order to keep doses to natives in the area below 3.9 roentgen, one would have to evacuate an area of 2,000 square miles, and I think they said something like an area of 500 square miles would have to be evacuated for a period of several months. So it will be quite necessary to take elaborate precautions. Again, these are based on assuming that one can predict well in advance just the way things might go. One might have to actually exclude greater areas.

There are ways in which certainly the AEC is working to maximize containment or minimize the amount of debris released. One is to attempt to use cleaner and cleaner weapons, but what cleaner weapons means at this time is more fusion and less fission, which means then more production of tritium which becomes a radioactive hazard that would be associated in waters that could be contaminated by debris.

The other thing that can be done is in increased containment by using less than optimum emplacement considerations, going to greater depth. But if one still wishes to come up with the same result in terms of the ditch that is being dug, if one goes to greater depth, one has to use larger explosives. So that even if the percent of venting becomes smaller at greater depths, it is a percent of a larger number and one may still have equal amounts of radioactivity.

So it is hard to see how they can completely contain radioactivity and still have a meaningful project that accomplishes the ends intended.

Senator MUSKIE. Eventually the crater has to be opened up.

Mr. KALKSTEIN. Right.

Senator MUSKIE. What is the next process then to insulate the crater and radioactivity contained within it with respect to the outside environment? In the case of the Panama Canal you have one elongated crater made up of a series of others, I assume from the Atlantic to Pacific. What dangers are involved in the opening up of that canal?

Mr. KALKSTEIN. It is very hard to be definite about specifically what will be there. I think we will have craters or maybe a lot of these will be row charges, ditches that need some connecting work. Certainly, as one would anticipate, there is some release of radioactivity. This would mean that some of the radioactivity gets into the atmosphere, some of it falls back, some radioactivity is attached to the surface of the ditch.

For one thing I am sure the AEC would plan not to send workers back into the ditch until the external dose levels are such that they are considered safe in terms of industrial or occupational hazard rather than public hazard. I am just not sure what they can do with regard to the surface to minimize leaching out of activity once the water is running through. My guess would be that their attempt would be to wait until the radioactivity has decayed to a sufficiently low level so that once one introduces the water in the canal dilution will take care of the problem. But one will be left with radioactivity.

One is also potentially left not just with the surface considerations, but the fact that there are lots of questions with regard to slope stability after one has created a crater. We have set off one series of explosions and formed the ditch we want, but what happens to the walls of that ditch when the next series goes off? I think one could contemplate that there will be a fair amount of work to sort of refashion the final work up of the whole pathway, so that it is in the configuration that they want finally.

This will at least mean a few people who admittedly are being paid to do this who are working with radioactive debris in the confines of the crater.

Senator MUSKIE. It seems to me there is another problem. The test is conducted under conditions as carefully safeguarded as possible, and I assume as carefully contained as possible. But are there any test conditions that will even approximate the kind of situation you have in excavating the Panama Canal, for example?

Mr. KALKSTEIN. No. The work you referred to that I did a summer ago on international arrangements and controls of peaceful applications raises this question. I think that what we have in our testing program or what we will get is answers relevant to Nevada, New Mexico, or wherever we carry out our tests. I think just as with the underground engineering for "Gas Buggy," when we really wanted to know what happened when we applied these applications, we had to go to the area where they were making the applications and not do the applications, but conduct tests in those areas. So, in terms of timetables for this, we don't have the answers in Nevada, or wherever we are presently conducting cratering experiments. I think, since the media will be different generally wherever one wants to conduct an application, one will have to have some testing program in those areas, too.

One of the things I think is significant and needs to be watched, and probably ought to be one of the concerns of the Commission in evaluating such programs as the nuclear explosives program and concern for public safety is that we are not anywhere near in a position to have answers on Plowshare with regard to canals, and yet the Interoceanic Study Commission is scheduled to come up with their conclusions by December of 1970.

It seems to me that it would be impossible at this point for them to recommend proceeding with a canal excavation by nuclear techniques. As I suggest, there are enough problems here that I guess many years are still necessary even for testing.

The testimony in previous hearings given by representatives of the AEC, the statement by United States and Soviet representatives to Europe this April in talking about Plowshare-type events, suggests we are at least 5 years away still from these results.

So I think particularly on the basis of the considerations of the hazards, even these we only will understand when we are done. We don't have the answers elsewhere. They would be necessary, and this then adds to whatever the general view of rate of progress we can make. I think we are a long way from having the answers and even further from really being in a position to say that these applications will pay off.

It sort of gets back again to the suggestion I made earlier that what would seem to be a sensible approach at this point would be to al-
accept a moratorium or suspension of the Plowshare program to really study
low for a commission such as this legislation suggests to really study
these aspects in detail and also to allow for the sort of international discussion that is necessary, to see if these objectives really make sense, not just in terms of satisfying a particular mission or developing particular technology, but protecting the needs of people in this country also.

ENVIRONMENTAL RISKS OF PLOWSHARE

Senator MUSKIE. One final question which you may or may not be interested in answering at this point: Are you pessimistic about the possibility of a fully operational Plowshare program without unacceptable risks of radioactivity?

Mr. KALKSTEIN. It is hard to say. I think part of the whole debate—and we had several years of it before the test ban moratorium in 1958 and during the period of moratorium before the limited test ban treaty—was that you had both sides of the debate admitting that there was hazard from fallout, each side had its own view as to what is acceptable or not acceptable.

To some people a remote possibility of real danger, even to a few individuals, ought to be considered enough to make something unacceptable. There are other people who don't quite look at it that way. My own feelings about the future of Plowshare are that it is something certainly in this instance where we could well take our time so that we at least are in the best possible position with regard to knowing what we do before we do it, with regard to knowing the consequences, and with regard to being able to weigh the consequences.

I would say certainly at this time the feasibility, the economic advances, and such, to be gained from Plowshare leave enough questions

as to whether or not there would be these advantages. So that concern for the safety aspects ought to now be overriding.

Whether or not in time the objectives look so desirable that many nations would agree that a careful program of testing evaluation be carried out, that at some time one can see if there would be a real payoff, I sort of feel is a little premature to try and predict.

Senator MUSKIE. Thank you very much, Dr. Kalkstein, for a very helpful session. I would like to go on but we do have one other witness. Your prepared statement will be included in the record.

Mr. KALKSTEIN. Thank you, sir.

(The statement referred to follows:)

PREPARED STATEMENT OF PROF. MARVIN KALKSTEIN

I welcome the opportunity to present testimony in support of a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation and other purposes. Beginning with the first application of nuclear technology almost 25 years ago with the bombing of Hiroshima, we have experienced an enormous growth in the application of new technology with very little public awareness and even less public control. Many of these applications have had significant impact upon our environment, often in completely unanticipated ways. Generally, many of these applications have been zealously advanced by industrial enterprises and private and public agencies in single-minded pursuit of the goals and purposes of those enterprises and agencies. We have seen within this quarter of a century the growth of public agencies that have developed a mission and a purpose of their own rather than remaining the guardian of the public interest. It is important that future applications of technology be viewed in their broadest terms with regard to their total impact upon society and our environment rather than in terms of the narrow interests of the agencies involved. It is not enough that these activities are conducted under civilian agencies. It is essential that they be brought under public control. In dealing with the activities of government agencies, Congress should be the obvious representative of the public in the exercise of public control.

The proposed congressional action to establish an independent commission to evaluate the uses of nuclear explosives would be a useful first step toward governmental responsibility in a limited but potentially significant area of technology. Since the first nuclear weapons were detonated, it was recognized that there were serious hazards associated with their use. Largely in recognition of these hazards and principally because of the radiation hazards from fallout, we now have the Limited Test Ban Treaty of 1963. However, even prior to the agreement to limit nuclear tests to underground testing, it had also been recognized by scientists that many of the same hazards still exist even when nuclear tests are conducted underground. Even for nuclear test conducted deep underground, there remains the seismic effects which can appear as earthquakes. Kenneth Pitzer, President of Stanford University and formerly chairman of an ad hoc committee on the safety of underground testing of the President's Science Advisory Committee, voiced concern about unresolved seismic hazards from underground nuclear tests and asked that possible test hazards be studied by an impartial judge and jury of experts who have no AEC affiliation. His position has been that this should be done before proceeding with large underground tests such as the recent nuclear test conducted in Amchitka, Alaska.

Potentially more serious and immediate hazards are associated with nuclear tests conducted in connection with the Plowshare nuclear excavation program. Cratering detonations used for such excavation purposes are conducted in such a way that the earth's surface is ruptured and radioactive material is released into the atmosphere. Edward A. Martell of the National Center of Atmospheric Research has concluded in a recently published paper in the April 1969 issue of *Environment* that sizeable fractions of the radioactive material are released to the atmosphere with a high degree of enrichment of strontium–90, strontium–89, and cesium–137 in the escaping cloud. These particular activities are of long

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lifetimes and have particular biological significance as they readily enter into the biosphere and find their way into man. Germain and Kahn of the Lawrence Radiation Laboratory in "Phenomenology and Containment of Underground Nuclear Explosions," UCRL 50482 (Nov. 1968) indicated that up to that time there had been venting of radioactive debris from 10 United States tests conducted since the Limited Test Ban.

Amounts of radioactivity released in these events range from 200 to 1 million curies estimated at 12 hours after the test. One million curies of radioactivity is comparable to the amount of radioactivity released in the above ground explosion of a 20 kiloton nuclear weapon, such as the one dropped on Hiroshima. In some respects the hazards from the radioactivity released in the venting of underground nuclear tests may be more serious than from atmospheric tests since the venting debris will tend to remain in the lower atmosphere and to be deposited sooner and in a more concentrated area after the test. Ferber and List of the Environmental Science Services Administration in a paper entitled "Prediction of External Gamma Dose From Nuclear Excavation of a Sea Level Canal" published in *Bioscience 19*, 234–237 (Mar. 1969) this year predicted that for the excavation of Route 17 of the Panama Canal, an exclusion area of about 5,000 miles of land area would be necessary during the operation. Because of the hazards involved, it would also be necessary to evacuate the population of a 2,000 square mile area and an area of 500 square miles would not be safe for re-entry for more than a month after the operation. In addition, there is potential air blast damage to distances of 60 miles or more. Martell in his paper estimated that over 30,000 inhabitants of the Canal Zone would have to be resettled.

Even "contained" underground explosions release gaseous radioactivity products to the atmosphere. Radioactive materials ejected into the atmosphere remain within the atmosphere for periods of time up to several weeks and have the potential of circulating the globe in that time. The question of detecting debris beyond one's boundary, should any venting occur, is primarily a matter of the degree of technical sophistication and effort that one might wish to bring to bear. It is clear that whether or not such debris might be detected, the cratering experiments would represent a violation, if not of the letter at least of the intent, of the Limited Test Ban Treaty which prohibits tests from which radioactive debris may pass beyond the boundaries of the territory upon which the test is conducted. The Chairman of the AEC during the hearings on the Limited Test Ban in 1963 conceded that within five or six years (or by 1969) further testing for earth-moving purposes (cratering explosions), would probably not be able to be conducted under the conditions of that Treaty. Clearly, both to honor our Treaty commitment and to safeguard our population and our environment, we should refrain from further underground nuclear tests until the commission proposed in this legislation has had a chance to study all the safety aspects of this problem so that we can be assured of the integrity of our environment.

A final area of concern with regard to the creation of a sea level canal using nuclear weapons, although of equal concern no matter what specific technique is applied for the purpose, is the potential ecological damage that can be done in the process. The introduction of foreign species from one environment to another (of the Atlantic to the Pacific Ocean or vice versa) can have as yet unknown consequences. An example of the potential dangers that exist even on a much smaller scale can be seen by the invasion of the lamprey eel into the Great Lakes, caused by the opening of the Saint Lawrence Seaway, and the subsequent devastation of the fishing industry on these Lakes.

If we are to protect our environment and to control the applications of nuclear technology, it is important that Congress review the nuclear testing policy of the United States. We have been testing nuclear weapons for a quarter of a century with relatively little questioning of the true purposes and effects of these tests. Nuclear test policy cannot be accepted solely as the province of the Atomic Energy Commission, which in the past has shown a lack of candor and concern in so far as the public interests have been involved. Nor can Congress leave its concern solely to the Joint Committee of Atomic Energy. It is time that Congress begin, as is being done now by this committee, to seriously examine this issue.

(Subsequent to the appearance of Prof. Kalkstein the following letter was sent:)

JANUARY 23, 1970.

DR. MARVIN KALKSTEIN,
Professor, State University of New York,
Stoney Brook, N.Y.

DEAR DR. KALKSTEIN: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass over any of the attached which are outside your area of specialty or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(The questions referred to and the answers received appear in the appendix IV, p. 445.)

Senator MUSKIE. Our next witnesses are John W. Gofman and Arthur R. Tamplin. You may proceed, gentlemen.

STATEMENT OF JOHN W. GOFMAN AND ARTHUR R. TAMPLIN, DIVISION OF MEDICAL PHYSICS (BERKELEY), AND BIOMEDICAL RESEARCH DIVISION, LAWRENCE RADIATION LABORATORY (LIVERMORE), UNIVERSITY OF CALIFORNIA

Mr. GOFMAN. Senator Muskie, we are well aware of the specific subject matter of your subcommittee's deliberation and because we are aware of it, we would like to have the general subject of our testimony be the Federal Radiation Council guidelines for radiation exposure of the population at large—protection or disaster?

We wish to apprise you that, in our opinion, the most crucial pressing problem facing everyone concerned with any and all burgeoning atomic energy activities is to secure the earliest possible revision *downward*, by *at least a factor of tenfold*, of the allowable radiation dosage to the population from peaceful atomic energy activities. The Federal Radiation Council allowable dose of whole body ionizing radiation is currently 0.17 rads per year. We shall present to you hard evidence that leads us to recommend that this be reduced *now* to 0.017 rads or even less. And we shall present to you the estimated disastrous consequences to the health of the public if this recommendation receives less than immediate, serious attention.

THE FEDERAL RADIATION COUNCIL GUIDELINES*

There has been ample reason for skepticism concerning the FRC guides for many years.[1] In essence, this is the case because a really valid scientific justification for the allowable dose of 0.17 rads of total body exposure to ionizing radiation has never been presented. The general vague statement is usually repeated that the risk to the population so exposed is *believed* to be small compared with the benefits to be derived from the orderly development of atomic energy for peaceful purposes.

*See footnotes following statements, p. 73.

Dr. Brian MacMahon, professor of epidemiology at Harvard, writing as recently as early 1969, stated:

While a great deal more is known now than was known 20 years ago, it must be admitted that we still do not have most of the data that would be required for an informed judgment on the maximum limits of exposure advisable for individuals or populations.[2]

This is vastly different from the bland reassurance of the Federal Radiation Council guidelines. We find ourselves in general agreement with Professor MacMahon, except that we go further and feel the already-documented evidence amply justifies a drastic revision *downwards*—and now.[3 4]

There is an even more hazardous situation associated with the vagueness of the justification for FRC guidelines. This hazard has become apparent to us through extensive contact with people in radiation surveillance work, in the atomic energy industry, and in atomic energy laboratories. Widely prevalent is the notion that the existing standards have a wide margin of safety built in. Many such individuals refuse to believe that any responsible body would ever set a guideline dosage into the Federal statutes without a wide margin of safety.

How is it possible that our current Federal Radiation Council guidelines may have falsely lulled us into complacency? Let us trace the evidence and to stick with facts, let us restrict our considerations to two major effects of radiation upon humans, namely, cancer and leukemia—in this generation—that is effects upon those humans actually receiving the radiation. *Any conclusion* we draw concerning the hazard of the current radiation guidelines can only be amplified and buttressed by consideration of the additional burden of human misery associated with genetic defects, fetal deaths, and neo-natal deaths.[3] The case against perpetuation of the existing FRC guidelines is overwhelmingly strong just on the basis of the cancer-leukemia risk alone, without even considering the potentially much larger problem of effects upon future generations.

HOW DID COMPLACENCY ARISE

First of all, there once existed a very great paucity of data concerning the dose versus effect relationship between radiation and cancer or leukemia induction in man. Steadily, however, during these past 20 years, many of the holes in these data have been filled from a combination of several extremely important sources:

(*a*) Study of survivors of Hiroshima-Nagasaki by the Atomic Bomb Casualty Commission.

(*b*) Study of patients *treated* with radiation for nonmalignant diseases earlier in life and then developing cancer or leukemia.

(*c*) Study of children who commonly received irradiation to the neck area in one unfortunate era of American medicine.

(*d*) Study of the occurrence of lung cancer in uranium miners in the United States.

(*e*) Study of cancer and leukemia in children whose mothers had received irradiation (diagnostic) during the pregnancy.

As the early results started to come forth from the Atomic Bomb

Casualty Commission, it was that *leukemia* might be appearing more frequently in those persons irradiated in Hiroshima and Nagasaki. Attention became centered upon leukemia as a sort of "special" response to ionizing radiation and not much thought was given to other forms of cancer. From the ABCC studies[1] and from wholly independent observations,[2] it is now clear, and we believe no one disputes the estimate that, at least for total doses of 100 rads or more, the leukemia risk may be expressed as follows:

One to two cases of leukemia per 10^2 exposed persons, where each of them received 1 rad of total body exposure. This does not require 1 rad per year; rather, we are talking about the above rate of disease occurrence with a total integrated exposure of 1 rad. Furthermore, this incidence of one to two cases per 10^2 people per year persists for many years, once the latency period is over, ultimately declining somewhat, at least for chronic leukemia.[5] It is a known fact, from many observations, that leukemia or cancer is *not* an immediate response to radiation. There is a period of years (different for different forms of cancer) *before* the clinical disease is manifest. This period is called the latency period.

An incidence rate of one or two cases per million people per year *sounds* like a small number, especially when this number is viewed in isolation. Indeed, many have hastened to add that spontaneously, without any manmade radiation, leukemia occurs with a frequency of 60 cases per million per year, which makes it a relatively rare disease. So, one or two cases per year sounds small by itself, and sounds even smaller viewed against a spontaneous rate of 60 per million persons per year. And, as a result, with the *early* atomic bomb survivor data *only* showing leukemia, a widespread complacency set in concerning long-term effects of ionizing radiation, a complacency extending to high circles.

For two very major reasons, this error in thinking has *turned out to be what we would have to refer to as a mistake of the first order of magnitude.*

(1) Leukemia happens to show a shorter latency period than most other forms of cancer. Therefore, the reason it appeared *early* to be the *only* malignancy in the Hiroshima-Nagasaki survivors was simply that not enough time had elapsed for the other forms of cancer to manifest themselves.

(2) The *proper* way to look at the incidence rate of 1–2 per 10^2 persons per year from radiation and the 60 per 10^2 persons per year spontaneously is *not* in isolation from each other, *but in relation* to each other. Thus, viewed in this light, 1 rad of ionizing radiation *increases* the leukemia incidence between 1.6 and 3.3 percent. Or, we can state that the doubling dose for leukemia (namely, that amount of radiation which will double the spontaneous rate) is between 30 and 60 rads. (Doubling a spontaneous rate of 60 cases per million each year means producing an additional 60 cases per million per year.)

WHAT ABOUT OTHER FORMS OF CANCER?

It now becomes an issue of paramount importance to know whether other forms of cancer behave similarly in response to ionizing radiation. *Are* other forms of cancer describable by a fractional increase in occurrence rate per rad, and if so, how do the fractions compare with

those for leukemia? We need no longer speculate about such matters because *hard, incontrovertible data* are available for human cancers induced by radiation. These data represent *facts, not opinion.* Estimates are available for several forms of cancer from worldwide data, U.S. data, and from the studies by the Atomic Bomb Casualty Commission of survivors of Hiroshima and Nagasaki. Let us consider a variety of forms of human cancers. I would like to review for you the evidence to show you the wideness of the data concerning a variety of forms of human cancer. First, for:[2]

(a) Thyroid cancer

The Japanese data, primarily based upon adults, show an approximate doubling dose of 100 rads for development of thyroid cancer, or approximately a 1-percent increase in incidence rate of thyroid cancer in the population per rad of exposure of the population.[5]

We can arrive at the risk for younger people in the United States of America from two items of data.

(a) Pochin gives the figure of one case of thyroid cancer per 10^2 persons per rad.[6]

(b) Carroll et al. reported that the spontaneous incidence rate for thyroid cancer is ~5–10 cases per 10^2 persons per year in the age range of 10 to 20 years.[7]

Combining these two items of information, it is estimated that between 5 and 10 rads is the doubling dose for thyroid cancer in young people in the United States. This means a 10- to 20-percent increase in risk of thyroid cancer in the youthful population per year per rad of exposure. Thus, considering the youthful group (United States) and the adults (Japan), the range is between 1- and 20-percent increase in thyroid cancer per year per rad of exposure.

(b) Lung cancer

Estimates are available from several sources for radiation-induction of lung cancer. The ABCC studies in Japan indicate an approximate doubling of lung cancer incidence rate for 100 rads of exposure, or a 1-percent increase in risk of lung cancer in the population for an exposure of 1 rad.[5] The experiences of the uranium miners in the United States are complicated by two factors: (a) The dosimetry is poorly known and (b) many of the workers are still in the latency period.[8] What estimates have been made for the uranium miners suggest the doubling dose for lung cancer to be between 250 and 500 rads. If the correction for latency is estimated as twofold, the final estimate would be 125 to 250 rads as the doubling dose.[8]

Miller has questioned the Japanese data because of nonspecificity of the histology of the cancer cells.[9] On the other hand, the similarity of the ratio of lung cancer to leukemia in the Japanese as compared to the British patients studied by Court-Brown and Doll suggests the Japanese data to be quite reasonable.[10] As a compromise estimate, we shall average the Japanese and United States data, to obtain 175 rads as the estimate for the doubling dose for lung cancer, or a 0.6-percent increase in the annual incidence rate of lung cancer in the population per rad of exposure.

(c) Breast cancer

Breast cancer has been found to be radiation-induced in the Japanese studies.[5] The estimated doubling dose is approximately 100 rads for

breast cancer, or, again, a 1-percent increase in incidence rate per year of breast cancer in the population per rad of exposure.

(*d*) *Other forms of cancer*

From some important studies on humans receiving therapeutic radiation for the arthritislike disorder known as rheumatoid spondylitis, Court-Brown and Doll [10] have studied the subsequent occurrence of many forms of cancer in organs heavily exposed, *incidental* to irradiation of the primary disease in the spine. We don't know that all the heavily exposed regions received equivalent doses, but it appears reasonable to estimate that the *various* heavily exposed regions were within a factor of 2 on either side of the median value for the group. If we use Court-Brown and Doll's value for bronchiogenic cancer of the lung as a reference value, (and for this form of cancer we have used 175 rads above as an estimated doubling dose), we can then estimate the doubling dose for radiation for several additional cancers. Uncertainty of precise dose comparisons make these numbers uncertain by a factor of 2 or thereabouts either on the low or high side. We shall, therefore, not only show the estimated doubling doses for all these additional cancers, but also a range to take this dose uncertainty into consideration. Thus, we have for the following additional cancers:

Site of cancer	Doubling dose (rads)		Percent increase in incidence—rate per rad	
	Mean	Range	Mean	Range
Pharynx	40	(20 to 80)	2.5	(1.2 to 5).
Stomach	230	(115 to 460)	.4	(.2 to .8).
Pancreas	125	(60 to 250)	.8	(.4 to 1.6).
Bone [1]	40	(20 to 80)	2.5	(1.2 to 5).
Lymphatic plus other hematopoeitic organs	70	(35 to 140)	1.4	(.7 to 2.8).
Carcinomatosis of miscellaneous origin	60	(30 to 120)	1.7	(.9 to 3.4).

[1] Bone may possibly have received higher irradiation dose than other sites. If this were true, the estimated doubling dose is too low for bone.

Now we are in a position to summarize the radiation-induced cancers for all sites, utilizing all the data available.

BEST ESTIMATES OF DOUBLING DOSE OF RADIATION FOR HUMAN CANCERS AND THE INCREASE IN INCIDENCE RATE PER RAD OF EXPOSURE

Organ site	Doubling dose (rads)	Percent increase in incidence rate per rad
Leukemia	30 to 60	1.6 to 3.3.
Thyroid cancer:		
Adults	100	1.
Young persons	(5 to 10)	(10 to 20).
Lung cancer	~175	0.6.
Breast cancer	~100	1.
Stomach cancer	~230	0.4.
Pancreas cancer	~125	0.8.
Bone cancer	~40	2.5.
Lymphatic plus other hematopoetic organs	~70	1.4.
Carcinomatosis of miscellaneous origin	~60	1.7.

We read you these numbers for a very specific reason, to point out to you that for such an array of widely divergent organ systems, already including hard data for nearly all the major forms of human cancers, it is amazing, indeed, that there is such a small range for

the estimated doubling dose. Correspondingly, there is a very small range in the estimated increase in incidence rate per rad for these widely differing organ sites in which cancers arise.

The only number that is different, and that one indicates an even higher susceptibility to radiation induction of cancer, is for thyroid cancer induction in youthful persons under 20 years of age. As we shall see below, this is not at all surprising or inconsistent, for the data presented below suggest a very high sensitivity of embryos in-utero to irradiation, causing subsequent leukemia and cancer during early childhood. As we consider youthful people, they are even more sensitive per rad than are adults.

Furthermore, in some of these studies, aside from leukemia, the persons at risk were most probably still in the latency period when studied, so that full expression of the disease has not yet been reached. This would mean then an even smaller radiation dose is required to double the incidence rate, or expressed otherwise, the percent increase in incidence rate per rad of exposure is even higher than that tabulated above. We know, from extensive other data, that bone cancer and skin cancer have definitely been produced by radiation. With further observation and study, the ABCC data will provide firm estimates of the doubling dose for the induction of cancer by radiation at the few remaining other major organ sites. At present the only malignant disease reputedly not induced by radiation is chronic lymphatic leukemia. And even this may be in doubt, since malignant lymphoma, a highly related cancerous disorder, is radiation induced, both from the data of Court-Brown and Doll [10] and from Japanese data.[11]

IN UTERO RADIATION AND SUBSEQUENT DEVELOPMENT OF CHILDHOOD LEUKEMIA AND CANCER

Further, let us consider the in-utero-radiation situation. Stewart and coworkers originally [12] and MacMahon [13 14] and Stewart and Kneale [15] recently have presented evidence that implicates in utero radiation of embryos (carried out for diagnostic purposes in the mother) with the development of subsequent leukemia plus other cancers in the first 10 years of life of the child. The general estimate of the amount of radiation delivered in such diagnostic procedures is 2 to 3 Rads to the developing fetus. From the Stewart and Kneale data, we have, for the following forms of cancer, the estimates of the increase in numbers of cancers for several organ sites:

	Percent
Type of Cancer:	50
Leukemia	50
Lymphosarcoma	50
Cerebral Tumors	50
Neuroblastoma	60
Wilms' tumor	50
Other cancers	

From the MacMahon data, we have the following highly similar estimates:

Radiation induced increase over spontaneous incidence

	Percent
Type of Cancer:	50
Leukemia	60
Central nervous system tumors	40
Other cancers	

If we now take the central values from both the MacMahon evidence and the Stewart-Kneale evidence, we have as a best estimate, 50% increase in incidence rate for all forms of cancer plus leukemia, associated with diagnostic irradiation of the infant in uetro, and the numbers are closely similar for U.S. practice and British practice. So, for 2–3 Rads to the infant in utero, a 50 percent increase in incidence rate of various cancers leads to an estimate of 4 to 6 Rads as the doubling dose for childhood leukemia plus cancer due to diagnostic irradiation in utero. Let us underestimate the risk, and use the higher number, 6 Rads, as the doubling dose for in utero induction of subsequent leukemia plus other childhood cancers. This means a 17-percent increase in the incidence rate of such leukemia plus cancers per Rad of in utero exposure of the infant.

It is not at all surprising that infants in utero should appear most sensitive to irradiation, children next in sensitivity, and adults third (but by no means low). This is precisely the order in which these groups stand in terms of the fraction of their cells undergoing cell division at any time—and much evidence suggests these are the cells most susceptible to cancer induction.[18]

We would like to propose at this time some general laws, not those statutes set by Congress, but some scientific laws of cancer induction by radiation.

GENERAL LAWS OF CANCER INDUCTION BY RADIATION

In view of the widely diverse forms of human cancers plus leukemias showing such striking similarity in their risk of radiation induction, it does not appear at all rash to propose these fundamental laws of cancer induction by radiation in humans:

LAW I: "All forms of cancer, in all probability, can be increased by ionizing radiation, and the correct way to describe the phenomenon is either in terms of the dose required to double the spontaneous incidence rate of each cancer or, alternatively, as the increase in incidence rate of such cancers per Rad of exposure."

LAW II: "All forms of cancer show closely similar doubling doses and closely similar increases in incidence rate per Rad."

LAW III: "Youthful subjects require less radiation to increase the incidence rate by a specified fraction than do adults."

Based upon these laws and the extensive data already in hand and described above, the following assignments appear reasonable for all forms of cancer.

For adults	~100 Rads as the doubling dose ~1 percent increase in incidence rate per year per Rad of exposure
For youthful subjects (<20 years of age)	Between 5 and 100 Rads as the doubling dose Between 1 and 20 percent increase in incidence per year per Rad of exposure
For infants in utero	~6 Rads as the doubling dose ~17 percent increase in incidence rate per year per Rad of exposure

For the radiation of infants in-utero, Stewart and Kneale[1] clearly stated the outlines of these general laws. For adults, Court-Brown and Doll[19] should be clearly credited with first having stated the outline of these general laws.

With all the additional data available plus the data of Stewart and Kneale, MacMahon, and Court-Brown and Doll, we consider the enunciation of these general fundamental laws as having a better experimental base than many laws of physics, chemistry, or biology had when first proposed. Furthermore, we would estimate that the absolute numbers, if anything, probably underestimate the risk. For purposes of setting radiation tolerance guidelines, one might even be advised to use lower doubling doses than those estimated above.

THE IMPLICATIONS OF THESE LAWS FOR THE POPULATION EXPOSURE ASSOCIATED WITH ATOMS-FOR-PEACE PROGRAMS

The statutory allowable dose to the population-at-large in the United States of America is 0.17 rads per year from peaceful uses of atomic energy in all forms. If everyone in the population were to receive 0.17 rads per year from birth to age 30 years, the integrated exposure (above background) would be 5 rads per person. If the risk for all forms of cancer plus leukemia is an increase of 1 percent in incidence rate per rad—and mind you, we have used the adult figure, not the childhood figure so we are being conservative—we have 5 by 1 equals 5 percent increase in incidence rate for all forms of cancer plus leukemia per year.

For a population of 2 by 10[8] persons in the United States of America one-half can roughly be estimated to be over 30 years of age. In this group, irradiated from birth, the latency period might, on the average, be expected to be over ~35 years of age.

The *spontaneous* cancer incidence is ~280/10[5] persons per year; 5 percent by 280 equals 14. Therefore, 14 additional cancer cases per 10[5] persons per year due to irradiation.

Thus, 14,000 additional cancer cases per year in the United States, considering only those over 30 years of age.

If we estimate that latency plus lower accumulated dosage provides a smaller number of additional cases in the under 30-year age group, it would by no means be an overestimate to add 2,000 additional cases for the under 30-year age group. (Especially is this true when we see the data above concerning the greater sensitivity of this group to radiation-induced cancer.)

There should be added some contribution of additional cases each year to take into account the fact that 0.13 rads will have been received by each infant in-utero. (0.17 rads/year by 40/52 years.) It is hard to know whether this in-utero radiation carries an increased cancer risk for the whole lifetime or not. The additional contribution for the in-utero radiation (at a period when the effectiveness per rad is very high) could be between a few hundred and several thousand additional cancer cases per year. We shall not attempt to guess the additional contribu-

[1] Stewart, A. and Kneale, G. W. "Changes in the cancer risk associated with obstetric radiography" *Lancet 1*, 104–107, 1968.

tion due to in-utero irradiation; and thus not knowing that number, we will just neglect it and stay with the 16,000 cases a year.

Therefore, 14,000 plus 2,000 equals 16,000 additional cancer plus leukemia cases per year in the United States if everyone received the Federal Radiation Council statutory allowable doses of radiation. This would, for the several reasons outlined, appear to be a minimum value; 16,000 cases is equivalent to the mortality rate from one recent high year of the Vietnam war. It would appear that this is rather a high price to consider as being compatible with the benefits to be derived from the orderly development of atomic energy.

And we must add to these estimates the comment that we have used only the hard data in hand based upon cancer and leukemia induced in humans by radiation. We have said nothing of the additional possible burden of loss of life and misery from genetic disorders in future generations, fetal deaths, and neonatal deaths.[8] Furthermore, we have not used the vast array of experimental animal data which indicate that not only does cancer mortality increase from irradiation, but that many, if not all, causes of death increase—and in about the same proportion as does cancer mortality. That would make the number we have indicated even four times higher.

WHAT MUST BE DONE

In the absence of any direct evidence in man that factors will operate to reduce these estimated cases of cancer plus leukemia, it would appear that the only sensible thing to do right now is to reduce drastically the Federal Radiation Council dose allowable to the population at large by at least a factor of 10. The new figure should be below 0.017 rads for peaceful uses of atomic energy. We are well aware that this suggestion recommends that manmade radiation exposure be limited to a small fraction (0.1 or less) of natural background sources.

ARE THERE ANY COUNTER-ARGUMENTS?

A number of counterarguments may be raised against this proposal by some advocates of the peaceful uses of the atom. Before demonstrating to you the lack of validity of every one of these arguments in turn, we must emphasize that this is not a proposal against peaceful uses of the atom. Rather, it is a proposal for the use of commonsense discretion in atomic energy development, weighted always in favor of the health and welfare of the people of the United States of America.

ARGUMENT 1: *"Atomic energy projects thus far have not delivered 0.17 rads to everyone in the population."*

That is perfectly true. But the nuclear power industry is only now getting going, and 0.17 rads per year is on the Federal statute books as allowable. Additionally, Plowshare proposals and industrial uses of radiation sources will surely add some increment to the population dosage.

ARGUMENT 2: ***"We don't plan to deliver the allowable 0.17 rads per year to everyone in the population at large from peaceful uses of atomic energy."***

Our only answer to that is we should certainly hope not. But, if it be true that such doses are unnecessary in the peaceful development of

atomic energy, and if it be true that we can develop atomic energy for electric power and other uses with a much lower delivery of radiation to humans, that is indeed excellent news. Surely there can be no objection to immediate codification of this welcome news into law so that no one can possibly be confused by a high allowable standard and the concomitant promise that we will stay well below that figure.

We have alluded previously to our experience indicating that misinformation concerning potential hazard is widespread, with numerous highly responsible people in atomic energy development laboring under the impression that the current standards have a wide margin of safety built in. Just recently an eminent authority in nuclear safety, Prof. Merrill Eisenbud, expressed his opinion that, "The standards contain enormous built-in conservatism" and "that 50 to 100 millirads per year (one-third to one-half FRC guideline values) will produce no harm."[17] We would indeed be relieved of our concern if Professor Eisenbud would replace his opinion with some hard evidence to refute the facts presented here today.

Industry urgently needs a real standard that can be expected to hold up over time, since a later revision downward can lead to excruciatingly costly retrofits in a developed industrial application of nuclear energy. It is far better to lower the guidelines for radiation exposure now and do our engineering accordingly. We believe engineering talent can direct its effort to essentially absolute containment of radioactivity at every step in any useful atomic energy development.

If we are fortunate enough later to find that some unknown effect operates to protect against the hazards we have demonstrated here, it will be easy enough to raise the guidelines for radiation exposure then. In this way we can avoid irreversible injury to our environment and to a whole generation of humans while we find out the true facts.

ARGUMENT 3: *"We live in 'a sea of radioactivity' and man has for time immemorial been exposed to ionizing radiation. Why worry about adding a little?"*

This argument presumes that natural radiation does no harm! As we can demonstrate readily by elementary arithmetic, natural radiation, in all likelihood, does just about as much harm as we would expect from all the evidence we have laid before you.

Let us apply our factor of a 1-percent increase in cancer incidence rate per rad. A reasonable value for average radiation due to natural causes is approximately 0.1 rad per year. At 30 years of age, the average man has received 30 by 0.1 equals 3 rads of radiation from natural sources. (It is higher in some locations, and we shall consider that in a few moments.)

Now 3 by 1 equals 3, so we expect a 3-percent increase in the spontaneous cancer rate due to natural radiation. We doubt very, very much that many persons informed in this field would be prepared to argue that 3 percent of spontaneous cancer plus leukemia could not be due to natural radiation. So, this argument concerning the sea of radioactivity falls of its own weight.

ARGUMENT 4: ***"But possibly there is a 'threshold' dose of radiation below which no harm accrues to man. Aren't you, therefore, unduly pessimistic about our standards?"***

There are two crucial answers to this question.

1. Before the work of Stewart, Kneale, and MacMahon all the data concerning cancer plus leukemia induction in man was for total doses of 100 rads or more. But their data for irradiation of infants in-utero are for 2 or 3 rads. And, even more importantly, their data indicate that each rad may be even 10 times more effective in inducing cancer at these extremely low total doses than is each rad at the high doses. So the threshold concept has suffered some rather severe reverses!

2. We and others are doing experiments on human cells actively to determine the effect per rad at various total doses to see if threshold type effects ever exist for man. But to use a hope based upon future experiments that such thresholds may turn out to exist in setting guidelines for the exposure of our population now would seem like absolute folly.

ARGUMENT 5: "*But isn't it true that delivering radiation slowly over a period of years, as would be the case for peaceful applications of atomic energy, may be less harmful with respect to cancer induction than the same dose delivered rapidly?*"

Maybe. It is perfectly true that, for some biological effects, the ability of the body to repair damage from previous radiation makes the effect of slow, protracted radiation less than for the same dose delivered rapidly. No evidence exists for such an effect on cancer or leukemia induction by radiation in man. Furthermore, the uranium miners received their irradiation slowly over a period of years, and it appears that any protection this provides, if there is any, is not enough to appreciably alter any of our major conclusions.

Further, it may take 10 or 20 years to ascertain whether such protraction of radiation lessens cancer induction in man. This only militates in favor of reducing the allowable dosage standards rather than against reducing them. Why, during such an interval of 10 to 20 years, should we take the high risk, at the expense of the people of the United States of America, of producing extensive and, in your words, irreversible injury?

ARGUMENT 6: "*But isn't it true that some children have received large dosages of radiation to their thyroid gland from radio-iodine from fallout, as in St. George, Utah, and have failed to show a high incidence of thyroid cancer?*"

Let us look very closely at this issue. Tamplin has presented evidence, never refuted, that high levels of radio-iodine were indeed deposited in the St. George area during the Nevada tests above ground during 1952–55.[18] If children in that area consumed 1 liter of milk each day from cows grazing upon contaminated pastureland, he calculated that the radio-iodine dosage to their thyroid glands would have been approximately 120 rads. Now there are several points to consider:

(*a*) There are some 2,000 children in St. George, Utah.

(*b*) When these children were examined, years after the possible exposure, some of the children in St. George were those who had moved there since the exposure, so the true number who might have been exposed is less than 2,000.

(*c*) Some of the children probably didn't drink 1 liter of milk per day.

(*d*) Some of the cows were not grazing on contaminated pastureland. They were eating uncontaminated stored feed.

But, for the sake of argument, let us assume to make the case exaggerated, all 2,000 children were in St. George, and did drink 1 liter per day of radioiodine-contaminated milk, and did receive 120 rads to their thyroid glands. How much cancer should have been expected?

Again, by simple arithmetic, we can use the midfigure for increased incidence of thyroid cancer in children per rad as 15 percent of the spontaneous rate. If the spontaneous rate is $\sim$10 cases per million per year, our expectation would be, for St. George:

$$\left(\frac{2,000}{10^6}\right)\times\left(\frac{15}{100}\right)\times\left(^{10}\right)\times\left(^{120}\right) = 0.36 \text{ cases per year.}$$

Thus, every 3 years, one case of thyroid cancer would be expected. With this expectation, one could go 6 or 10 years and not see that one case. Further, the points mentioned above in (b) through (d) would have reduced even this small expectation. So the data from St. George, Utah, does not prove at all that radioiodine exposure does not produce cancer in children. The St. George studies just prove if an inadequate study is done, an inadequate result is obtained.

ARGUMENT 7: "*But isn't it true that living in Denver at high altitude exposes people to more cosmic radiation and that as a result their annual natural radiation dose is 1.5 to 2 times what it is at sea level?*"

The answer is, "Yes."

"Then why don't they have a higher cancer incidence than people at sea level?"

This particular argument is brought out and burnished brightly at regular intervals.

The answer is that the excessive radiation due to cosmic rays probably produced precisely as much extra cancer in Denver as our calculations would indicate. Let us make those extremely simple calculations.

First, to compare Denver with a sea-level region, we would have to know that the medical reporting of disease categories were just as good both in Denver and the sea level community.

Second, we would want to be sure that the people at risk in Denver had lived there all their lives, and the people at sea level had lived there all their lives.

Third, we would want to be sure that all other factors, aside from radiation, were identical in Denver and the sea level community.

We don't know all these points, but let us suppose we were satisfied on all three. Let us say, to exaggerate the case, that Denver residents get 0.2 rads per year versus 0.1 rads per year at sea level. In 30 years, the average Denver resident would accumulate 6 rads; the average sea level resident would accumulate 3 rads.

Using our increase in cancer incidence rate of 1 percent per rad, we would estimate: For Denver, a 6-percent increase in the cancer incidence rate; for sea level, a 3-percent increase in the cancer incidence

rate. So, if we set all other "spontaneous" causes of cancer at 100 percent, we would say, Denver residents should experience 100 plus 6 equals 106.

Sea level residents should experience 100 plus 3 equals 103.

No expert in the field of vital statistics would be prepared to contest that Denver residents might be experiencing a 3-percent increase in cancer incidence rate due to cosmic radiation compared with otherwise equivalent people at sea level.

ARGUMENT 8: *"But aren't medical X-rays also capable of producing cancer along the lines of your argument?"*

Absolutely. There is no justification whatever for nonessential X-rays in the course of medical practice. Every physician should acquaint himself with the facts described above and he should be convinced that the risk to his patient is greater by not having a particular X-ray taken than by having it taken. There is ample evidence of a concerted campaign within the medical profession to reduce the radiation exposure through diagnostic X-rays.

ARGUMENT 9: *"Why do you criticize the guidelines for radiation exposure from the development of nuclear energy for electricity generation and say nothing of the hazard to the public from fossil-fuel electricity generating plants?"*

Our answer is that we don't condone homicide with knives any more than homicide with guns.

We are in the field of atomic energy and we believe our knowledge enables us to speak to the issue of atomic energy. Therefore, we are presenting the evidence upon which a reasonable set of guidelines for radiation exposure from the peaceful atom can be based. We are not against nuclear generation of electricity. We have great confidence that our engineers have the talent to design reactors, reprocessing plants for spent nuclear fuel, transport systems, and waste storage facilities in such a manner that any release of radioactivity that might conceivably expose humans be kept so low as to preclude harm.

If fossil-fuel plants are causing disease in our population, this issue should be evaluated as soon as possible, and the fossil-fuel generating plant should be redesigned to remove effluents that are producing harm.

The general argument that making either nuclear plants or fossil-fuel plants safe will increase the cost of electricity does not impress us. Probably a dollar per month added to electricity cost per family is an overestimate of what it would take, to allow superclean plants either of fossil fuel or atomic variety. We submit it is much better to pay a little more for electricity than to die prematurely of cancer or leukemia.

ARGUMENT 10: *"Experts have estimated that the dosage levels we are discussing in the existing Federal Radiation Council guidelines might only shorten the average lifespan of humans some weeks or months. Isn't this worthwhile compared to the benefits?"*

Absolutely not. First, even the average life shortening may be greater than estimated. Let us assume, however, that the experts are right. The real answer is that this argument is totally immoral. Let us assume

it is true that the average life expectancy is reduced only by several weeks. But how, we must ask, does this average reduction come about? Is arises because many of the victims of premature cancer—those 16,000-plus cases per year we referred to previously—lose 10, 20, 30, 40, or 50 years of their potential lifespan. While 16,000 cases is a large number, when it is diluted into the couple of hundred million people in the country, the resulting average reduction of lifespan due to radiation-induced cancer comes out only several weeks. This monstrous hoax should stop recurring.

SOME CLOSING REMARKS TO SENATORS MUSKIE, GRAVEL, RANDOLPH, AND THEIR COMMITTEE COLLEAGUES

We believe the real area where the problem of safeguarding the public health rests is in the primary biological standards of allowable radiation exposure. We do not think the current standards are at all acceptable.

With respect to calculating how much radiation the public might receive from nuclear power reactors, underground Plowshare events, we have, in our own laboratory—supported by the Atomic Energy Commission—developed, under Dr. Tamplin's and Dr. Ng's guidance, a handbook, "Prediction of the Maximum Dosage to Man From the Fallout of Nuclear Devices," which enables anyone to calculate the radiation dose possible to any organ of the body from each and every radionuclide prducible.[19] Wherever the data are inadequate, the worst possible case is assumed, in order to err always on the side of public protection. We constantly are updating this handbook and are providing it to workers in the atomic energy field nationally and, where requested, worldwide. We welcome anyone concerned including, of course, members of your committee, to visit our laboratory to learn, in detail, how to use this handbook for their needs in public protection work in the radiation field. Thus, in the area of estimating possible dosage to humans, there exists, sponsored by the Atomic Energy Commission and required for its work, a highly advanced ability for anyone who wishes to avail himself of it.

Additionally, in the supplementary section of this testimony is an extensive recent bibliography of contributions from our laboratory bearing directly upon documentation related to possible dose from underground nuclear explosives of the Plowshare program. We believe this committee will find that a large body of evidence is being developed already on this subject.

At the same time we, both members of an Atomic Energy Commission supported laboratory, should like to speak out on two issues of major importance.

First, any release of radioactivity associated with Plowshare or other programs to regions where humans or other members of our ecosystem can possibly be exposed should be documented by a truly independent agency and made immediately available to public sources for independent review. It may well turn out that attention to injury to other members of the ecosystem may be of greater long-range relevance to man than the immediate attention to man with extensive neglect of the ecosystem which supports his life.

The U.S. Public Health Service is, in principle, such an independent agency, but in practice the overly long delay in release of their measurements for public review is unacceptable. Furthermore, in the vicinity of the Nevada test site the AEC can exercise control over their reporting practices. This is also unacceptable. We understand some deficiencies in this regard have been corrected recently. All measurements of radioactivity releases, radionuclide by radionuclide, to any unrestricted area must be made available for public scrutiny on an immediate and, therefore, timely basis. It is doubtful that public credibility can be maintained under existing circumstances.

It is difficult to believe such requirements can really in any way compromise the national security. If measurements of radioactivity releases to unrestricted areas can possibly benefit an unfriendly power, it would indeed be a paradox that such measurements are possible for a hypothetical unfriendly power while being withheld where they may impinge upon the public health and safety of citizens of the United States.

Second, we are speaking out in the strongest terms against the current guidelines for radiation exposure to the population at large. We are urging the Atomic Energy Commission itself to join us in seeking early downward revision of the Federal Radiation Council guidelines.

When the AEC in 1963 requested our laboratory to undertake long-range, systematic studies of the effects of manmade radiation upon man, we told AEC Chairman Seaborg and then-Commissioner Haworth that the results of our studies could very well suggest restrictions upon on-going or proposed AEC projects. We said further that we intended fully to disclose publicly any evidence developed, favorable or unfavorable to the AEC. Both commissioners assured us they were perfectly happy about this prospect—all they wanted was for us to be sure to provide the truth.

Today, we have presented your committee with much evidence indicating that current radiation exposure guidelines are indeed dangerous—much too high. It would certainly be naive for us to believe that our recommendations will be received with enthusiasm in all quarters. To the best of our ability we have endeavored to present the truth. Our calculations, our evidence may, upon critical examination by others, prove wrong in minor respects. We doubt they will prove wrong in any major respect. But the sharp cutting edge of scientific criticism, with all the evidence placed squarely in the open forum, will demonstrate any fallacies, will show where additional evidence is needed, and where errors have been made by us.

We intend to continue to provide critical appraisal of questions of atomic energy safety in such a manner that the evidence can be examined by the scientific and public community at large. We do not subscribe to the concern that the public might, thereby, become unduly or prematurely alarmed. If a real controversy concerning the evidence exists, the public very well ought to be alarmed, and ought to demand that we pace our technical progress in such a way always that unanswered questions are decided in favor of the health and welfare of the public.

(Footnotes included in statement of Mr. Gofman and Mr. Tamplin follow:)

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Supplement to the Testimony of John W. Gofman and Arthur R. Tamplin

In our prepared statement we indicated that in our own Laboratory, sponsored by the Atomic Energy Commission, an advanced ability for *prediction* of estimated dosage from nuclear events is available. Included here is a non-exhaustive, but recent, bibliography of direct *documentation* studies of the distribution of radioactivity from Plowshare nuclear explosions and related studies from our own Laboratory. We believe that this Committee will find useful in its deliberation an overview of the kinds of efforts already completed, or in progress, related to the subject of Committee inquiry.

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Before we conclude I will, without objection, place in the record at this point a communication and resolution from the Federation of Western Outdoor Clubs.

(The information follows:)

FEDERATION OF WESTERN OUTDOOR CLUBS,
November 14, 1969.

Senator EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, New Senate Office Building, Washington, D.C.

DEAR SENATOR MUSKIE: In connection with your Sub-Committee hearings next week on S. 3042, I enclose copies of a resolution recently passed by the Federation at its 1969 meeting, with respect particularly to nuclear testing on Amchitka Island in the Aleutians. The Federation represents some 100,000 members of 44 organizations throughout the west, who feel that more consideration should be given for the total environment, the whole ecology of regions, in governmental and other operations. For too long we have gone ahead with many actions, and called them progress, but without any real consideration being given to their long-range effects on our lives or on our environment, on which our lives depend.

We respectfully request that a copy of our resolution #3 be made a part of the hearing record as an indication of our position on nuclear testing in the Aleutians, as far as pertinent to the purposes of your hearing on this legislation.

Knowing of your interest in the many facets of pollution problems everywhere, I am also enclosing copies of our resolution on environmental poisoning. We feel strongly that though DDT and/or similar pesticides may be banned in the near future, there are many other toxic substances being discharged into streams and lakes which are responsible for adverse effects on fish and wildlife. Industrial residues must be governed by industry, either voluntarily or under enforcement, so that they do not have adverse impact on environment. We have recommended to Secretary Finch of HEW that, pursuant to his announced desire to work with Interior and Agriculture Departments on pesticide control, they also work together to establish environmental standards for toxic substances, and then create the necessary regulations or request whatever legislation they may feel necessary, then proceed with enforcement. Daily these pollution problems get worse, we can't afford to wait two years for action. So much is already irretrievable; the longer we delay, the more loss we will sustain, and the more likely that bad effects will begin to be felt by man.

Sincerely,

MRS. BETTY HUGHES, *Secretary.*

FEDERATION OF OUTDOOR CLUBS, RESOLUTION NO. 3, 1969—AMCHITKA ISLAND NUCLEAR TESTING

The Aleutian Island National Wildlife Refuge, of which Amchitka Island is a significant part, was set aside in 1913 for the purpose of protection of the outstanding wildlife resources of this beautiful island chain. Included also in the executive order establishing the refuge was a provision permitting limited military use of portions of it where essential for national defense.

Since 1965, the Atomic Energy Commission has literally "taken over" Amchitka Island for the purpose of conducting a series of nuclear tests. The latest of these tests, involving an explosion equivalent to nearly a million tons of TNT, is scheduled for October, 1969.

Both the nuclear explosions themselves, and the attendant construction, road-building, and service activities have severely damaged the ecological balance of plant and animal life on and near Amchitka. Wildlife species have been driven away; the tundra has been deeply scarred; and an incredible profusion of junk and litter lies across the landscape. We are informed that military activities are being considered for other islands within the Refuge, notably Agattu, a large island of the same high caliber as Amchitka.

The Federation believes that activities of the type now being conducted or proposed for Amchitka were obviously never envisioned, so cannot be authorized by the 1913 order, therefore are in violation of it. It deplores the continuing degradation of the fragile Aleutian environment resulting from such activities, and particularly opposes the use of a National Wildlife Refuge for such purposes.

It is therefore resolved, That the Federation of Western Outdoor Clubs expresses strong opposition to any nuclear testing activities on Amchitka Island, and affirms the principle that Wildlife Refuges across the nation should not be used for such purposes.

THE SECRETARY OF HEALTH, EDUCATION, AND WELFARE,
Washington, D.C., January 28, 1970.

Senator EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: This is in reply to your letter of December 1 pertaining to the testimony of Drs. Gofman and Tamplin for the hearings of the Subcommittee on Air and Water Pollution, on November 18.

Gofman and Tamplin, in reaching their conclusion that the Federal Radiation Council guidelines should be "reduced *now* to 0.017 rads or even less," used an approach similar in principle to that used by expert advisory groups (e.g. ICRP, NCRP, FRC) in developing radiation protection standards and guidelines. This approach is based on the assumption of a direct linear and non-threshold relationship between dose and biological effect. In contrast to Drs. Gofman and Tamplin, however, these expert groups generally agree that this approach probably overestimates the risks, but is the prudent one to use in the formulation of radiation protection guides.

While we concur with this basic approach, we do not agree with all the premises, conditions and extrapolations used by Gofman and Tamplin in their testimony. In general, we believe that their calculations result in overestimates rather than, as they indicate, "minimum values" of cancer risk. Nevertheless, we believe that there is a need to establish more definitive estimates of the radiation risks that are associated with assumed, or observed, exposure conditions; otherwise there is inadequate basis to evaluate benefit versus risk. We also agree with the concept that the radiation standards should be developed on the assumption that any increase in radiation exposure will be accompanied by a commensurate increase in the risk of cancer.

Drs. Gofman and Tamplin have raised the question of whether the present FRC guidelines are still acceptable. In the past ten years, since the formulation of the FRC basic guides, sufficient additional information has developed from epidemiologic studies and animal experiments so that a reevaluation of such guidelines is believed to be warranted.

In view of our concern with the potential hazard of ionizing radiation in the environment, and as Chairman of the FRC, I am recommending that the Council institute a careful review and evaluation of the relevant scientific information that has become available in the past decade. I am recommending that this reevaluation provide, as definitively as possible, estimates of the risks associated with low levels of environmental radiation as a basis for review of the adequacy of current FRC guidelines as applicable to projected radiation levels. Based on projected exposure classes of radiation sources, such as nuclear power reactors, other peaceful uses of nuclear energy, and radiation from consumer products would also be considered.

I hope that these comments are useful to your subcommittee. Please call on us if we can be of any additional assistance.

Sincerely,

ROBERT H. FINCH,
Secretary.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION, LAWRENCE RADIATION LABORATORY,
Livermore, Calif., February 19, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: We wish to express our deepest appreciation to you and your Subcommittee for the opportunity to have participated in the recent Hearings. The milestone achievement of your Subcommittee's efforts, culminating in Secretary Finch's announcement that the entire structure of Federal Radiation Council Radiation Guidelines will now be thoroughly reviewed is one of the most phenomenally important steps forward in Environmental preservation imaginable.

As citizens of the U.S. we can only express our gratitude that this achievement of your subcommittee has been made, and made so rapidly. We all realize how

little time is left for constructive action, and this is why your accomplishment must be regarded as being of momentous importance. The true impact of what has been accomplished will only be realized by people in the U.S. and the world for generations to come. Certainly in the effort to preserve a habitable environment for humans, no single accomplishment can match this step forward in radiation protection accomplished by you and your colleagues of the Senate Subcommittee on Air and Water Pollution.

We realize your task has been a gigantic one, and we understand the many obstacles that had to be overcome. We wish we could have been of more assistance, but at least for the minor role we did have in helping this great step forward, we are indeed grateful.

Respectfully yours,

ARTHUR R. TAMPLIN.
JOHN W. GOFMAN.

UNIVERSITY OF CALIFORNIA,
Berkeley, Calif., January 23, 1970.

Federal Radiation Council Review of Radiation Standards for Population Exposure

Memorandum to: Dr. Paul Tompkins, Executive Director, Federal Radiation Council.
From: Alexander Grendon.
Subject: The Federal Radiation Council Review of Radiation Standards for Population Exposure.

Drs. J. W. Gofman and A. R. Tamplin sent you a paper dated Dec. 26, 1969, under the above subject reference. This is intended as a rebuttal of that paper.

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INFERENCES FROM RADIUM EXPOSURE IN HUMANS

By Alexander Grendon, Donner Laboratory, University of California, Berkeley, California

In a paper dated December 26, 1969, entitled: "Studies of Radium Exposed-Humans II: Further Refutation of the R. D. Evans' Claim that 'The Linear Non-Threshold Model of Human Radiation Carcinogenesis is Incorrect'", Drs. John W. Gofman and Arthur R. Tamplin have indulged in the game of dissecting a straw man under the microscope. The game started with a paper entitled: "Studies of Radium-Exposed Humans: The Fallacy Underlying a Major 'Foundation of NCRP, ICRP, and AEC Guidelines for Radiation Exposure to the Population-at-Large'", which the same authors offered as a "Supplement to testimony presented before the Sub-Committee on Air and Water Pollution, Committe on Public Works, United States Senate, 91st Congress, November 18, 1969." Both papers are intended as a critique of "one of the *purported major foundations* of guidelines established by the ICRP, the NCRP, and the FRC" for radiation protection purposes.

The "major foundation" they attack is in fact only a hypothesis suggested by Dr. Robley D. Evans, one of the principal investigators of the effects of radium and related substances in man. Whatever the opinions of the members of ICRP, NCRP, and FRC may be about Evans' hypothesis, the hypothesis has never been accepted as the foundation of any suggested standards for radiation exposure of the public and has only been in the background when standards for occupational exposure to radium were developed. The data accumulated by Evans and others have indeed played an important part in setting standards for occupational exposure to radium and radionuclides of other bone-seeking elements.

It is unfortunate that Gofman and Tamplin do not represent Evans' hypothesis on the basis of his published papers but rather on the basis of some remarks he made in oral testimony before a Congressional committee. In his publications, Evans has suggested that extrapolation of the increasing period of

latency* as dose decreases indicated a "practical threshold," in that, when doses are below some particular level, the lag before induction of cancer may exceed the duration of life. By one method of extrapolation, for example, he derived estimates as follows:[1]

Dose, cumulative rads (CR)	*Predicted latency period, years*
700	45–110
620	50–125
500	60–150
400	70–175
250	100–250

In his oral testimony, Evans unfortunately berated other hypothesis and went beyond his cautious published statements to assert: "It is my conviction that there does exist an obsolute threshold and a practical threshold for inhaled radon daughters, below which these nuclides are innocuous." The statement in one of his published works[2] is more guarded: "In such cases [where "practical threshold" appears] the existence of an 'absolute threshold' for somatic effects becomes irrelevant from the standpoint of radiation protection. Indeed, 'absolute threshold' concepts probably are applicable only to immortal systems such as certain cell cultures or to strains of experimental animals carried through many generations of continued irradiation, and in which cellular repair or replacement and tissue or organ recovery can be demonstrated to keep pace with radiation injury."

The fact is that various hypotheses can be formulated which, when tested against the existing data, cannot be rejected on statistical grounds. The methods of statistics do not "prove" relationships; they can merely offer a basis in probability for *denying* asserted relationships. If a hypothesis withstands repeated attempts at such denial in a number of experimental or epidemiological trials, it may claim to be more and more strongly supported by the evidence. There is no more basis in the existing data on radium-exposed humans for denying the hypothesis of an "absolute threshold" than for denying a hypothesis of linear proportionality.

Gofman and Tamplin have gone to great pains to calculate, point by point, what their hypothesis would predict as the number of cancer deaths and to show that statistical theory affords no basis for rejecting *their* hypothesis. That is undoubtedly true, but it does *not* constitute a test of *Evans'* hypothesis, as they continually allege. (For example, one out of many statements: ". . . even this extreme gesture leaves the Evans' concept unsupportable."[3] Two defects of Gofman and Tamplin's analysis are crucial and each will be discussed:

1) They do not give proper consideration to Evans' hypothesis about the relation of latency to dose.

2) They perform their analysis on the basis of their hypothesis that all cancers are similarly increased by radiation without openly noting that this assumption is irrelevant here.

With regard to Point 1):

Gofman and Tamplin state[4]: "We agree [with Evans] that it is reasonable to assume longer latency period at the lower dosages." They then assume periods ranging from 10 years for doses above 2500 rads to 25 years for doses down to zero (i.e., the interval, 0–100 rads). Contrast this schedule with Evans' extrapolations cited above or with the following facts observable in Evans' Figure 5,[5] which they cite as their source. Evans' Figure 5 shows 64 cases studied with doses from 100 to 1,000 cumulative rads (CR) and a range of time since exposure which, except for one subject who died of other causes 20 years after first exposure, ranges from 37–52 years. None of these had a tumor at that time, so that true latency period is indeterminate. Gofman and Tamplin suggest

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*"Period of latency" refers to the interval between exposure (often, the beginning of an accumulating exposure" and occurrence of the overt disease.

[1] R. D. Evans, A. T. Keane, R. J. Kolenkow, W. R. Neal, and M. M. Shanahan in *Delayed Effects of Bone-seeking Radionuclides*, C. W. Mays *et al.*, Eds. (University of Utah Press, Salt Lake City, 1969), p. 172. The upper values of latency duration have been slightly reduced from those inferred by Evans.

[2] *Ibid.*, p. 179.

[3] J. W. Gofman and A. R. Tamplin, *Studies of Radium Exposed-Humans II: Further Refutation of the R. D. Evans' Claim that "The Linear, Non-Threshold Model of Human Radiation Carcinogenesis Is Incorrect"* (mimeo dated Dec. 26, 1969), p. 10.

[4] *Ibid.*, loc. cit.

[5] P. 167 of Reference (1).

20 years for this dose range. In the group with 1200–2500 rads (CR), the data show one subject died of other causes 14 years after exposure, 4 developed sarcomas after 28–32 years, 1 at 50 years, and 8 showed no tumors after 41–51 years. From these data, Gofman and Tamplin suggest a latency period of 15 years. They do not undertake an analysis of latency period, as Evans had done, and apparently seized upon some convenient numbers for calculation rather than consider whether prolonged latency may operate to afford the "practical threshold" that Evans predicts.

(With regard to Point 2):

Gofman and Tamplin have made the assumption, in prior papers, that all neoplastic processes are equally affected per rad of absorbed dose; i.e., that some such dose as 25 rads doubles all forms of cancer from then on, as compared with the "natural incidence" of each form. In a recent paper,[6] I have discussed reasons for rejecting that assumption Using that assumption as a starting point, they painstakingly compute a series of expectancies for each of 10 dose categories discussed by Evans. These calculations show that the data are not satistically inconsistent with their hypothesis; and they remark with unconcealed satisfaction, after comparing their calculated expectancy of 12.62 cases of cancer with the observed 10 cases: "Realizing the statistical error in this small number of observed cases, this agreement is nothing short of fantastic!"[7] Consider, however, the same sort of comparison omitting the "doubling dose" hypothesis and using the concept that the number of cases of cancer is proportional to the dose of radiation, with possible minor differences for different sites according to tissue sensitivities. Using the same estimate as is widely accepted for the incidence of radiation-induced leukemia, namely, 1–2 cases per million per year for each rad, starting after a latency period of about 5 years and fading out after 15–20 years, one might conveniently assume some 20×10^{-6} total cases per rad as the expectancy for cancers among the radium-exposed group. The estimate leads to an expectancy of 10.65 cases, which is even closer to the observed 10. There is nothing "fantastic" about this; it simply shows that, with so little data available, a firm basis for rejection of any of several hypotheses cannot be found.

If the choice among hypotheses had to rest on these data alone, one might toss a coin. The many other pieces of evidence, however, that support the two relevant hypotheses—1) that carcinogenic response to radiation is more nearly constant in terms of cases per rad than in terms of "doubling dose," and 2) that latency is prolonged inversely to dose more nearly as Evans' projection indicates than as Gofman and Tamplin's unexplained choice of numbers—lead to the conclusion that no immediate reason exists to depart from current standards. There are many other criticisms that might be directed in detail at the Gofman and Tamplin analysis of Evans' data; but the chief criticism must still be that they are completely mistaken when they assert[8]: ". . . this analysis demonstrates again the wide usefulness of the doubling dose concept in human radiation carcinogenesis."

Radiation Protection Standards

(By Alexander Grendon, Donner Laboratory, University of California, Berkeley, California)

[Submitted to *Science*, January 16, 1970]

Widespread concern over the standards employed in the regulation of sources of radiation has recently been aroused by a thesis presented by Drs. John W. Gofman and Arthur R. Tamplin of Lawrence Radiation Laboratory. Their reinterpretation of existing data on the biological effects of radiation has led them to conclude that the Federal Radiation Council (FRC) guidelines[9] for radiation exposure ought to be reduced by at least a factor of ten. They have presented

[6] A. Grendon, *Radiation Protection Standards* (submitted to *Science*, Jan. 16, 1970).
[7] P. 25 of Reference (3).
[8] *Ibid., loc. cit.*
[9] a. Federal Radiation Council, Report No. 1, *Background Material for the Development of Radiation Protection Standards* (U.S. Dept. of Commerce, Washington, D.C., 1960).
b. *Radiation Protection Guidance for Federal Agencies*, Federal Register, May 18, 1960, pp. 4402–3.

their thesis before the Institute of Electrical and Electronic Engineers[2] and before the U.S. Senate Subcommittee on Air and Water Pollution[3]; but those who have specialized in the appropriate scientific disciplines to examine their work critically have had no prior opportunity to do so. Several scientists have presented rather hasty and hence superficial rebuttals; others are at work on more detailed analyses and critiques. The early consensus of those scientists who have long worked in this field seems to be that Gofman and Tamplin have made erroneous inferences from the data. The consensus of those governmental officials who have been involved in regulation and control of radiation hazards at Federal and State levels is that Gofman and Tamplin have also made erroneous inferences about the regulatory processes, the regulations themselves, and their practical consequences. These two aspects—the scientific and the regulatory—must be considered separately to avoid confusion.

On the scientific side, one must note that Gofman and Tamplin, in pressing for a lowered guide level, make no claim that they have developed new data. Their contention is that new generalizations can be validly drawn from the existing data on which the International Commission on Radiological Protection (ICRP) and the National Council on Radiation Protection and Measurements (NCRP) have based their estimates of biological risk. These two bodies, composed of scientists in pertinent fields of radiation and public health, have long been the most authoritative sources of scientific information for the guidance of governments, our own and others. In recent years, the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR), which is composed of many of the same persons as the ICRP and NCRP, has contributed greatly toward documenting the scientific foundation of this field. Governmental regulatory agencies, at all levels and in all countries, have had to rely on these scientific groups and their publications when making their value judgments as to appropriate control measures.

The ICRP and NCRP, at the insistence of governments, have gone beyond the bare statement of the results of scientific studies and have proposed value judgments founded on those studies. In some instances, they have expressed their reluctance to make such judgments since the question at issue is the proper balance point between social benefit and risk of harm to people, and that question is not subject to scientific resolution. It is important, then, to note carefully where science stops and social philosophy begins.

Origins of Protection Standards

The balance point that is currently accepted by regulatory agencies in the United States is the result of an evolutionary process that began only a few years after the X ray and radium were discovered. The earliest rules sought only to prevent the obvious prompt harm from excessive exposure in the form of ulcerative burn injuries. Next came recognition of a delayed effect, in that repeated exposure to large doses of radiation was found to increase the risk of cancerous or neoplastic changes—the carcinogenic effect. Allowable exposures were reduced to an extent that was deemed to make this risk no worse for persons occupationally exposed to radiation than the industrial risks of other occupations. Indeed, until some 15 years ago, it was believed by most scientists that a very small amount of radiation involved no risk at all—that there was a threshold below which any effect was either nonexistent or at worst temporary and incapable of producing permanent harm, as has long been believed to be true of toxic substances taken into the body. That viewpoint began to change with studies of the effects of radiation on the hereditary material of those cells which create the offspring of the present generation of living things.

Genetic effects of radiation were first noted more than 40 years ago. They became the governing consideration in radiological health some 10 years ago primarily because, in this area of investigation, it began to appear that the concept of the existence of a threshold for radiation injury was doubtful and probably false. Experiments on animal species, starting with the fruit fly and continuing with mice and some other species, showed that genetic changes oc-

[2] J. W. Gofman and A. R. Tamplin. *Low Dose Radiation, Chromosomes, and Cancer* (to be published in IEEE Proceedings, February 1970).
[3] J. W. Gofman and A. R. Tamplin. *Federal Radiation Council Guidelines for Radiation Exposure of the Population-at-large—Protection or Disaster?* Testimony presented before the Sub-Committee on Air and Water Pollution, Committee on Public Works, United States Senate, 91st Congress, Nov. 18, 1969 (mimeo).

curred at all experimentally practicable levels of radiation and, as nearly as the earlier data could indicate, the number of detectible changes was approximately proportionate to the total dose. It was not practicable, of course, to conduct these experiments at the low levels of radiation with which we are concerned when discussing allowable human exposures, since the number of changes attributable to radiation at these low levels, assuming such changes do occur proportionately, would not be recognizable among the many precisely similar changes that occur for other causes, including natural or background radiation. At least, if such changes do occur at very low radiation levels, one would have to experiment with astronomical numbers of animals under very precisely controlled conditions and then apply refined statistical methods to the data to establish that fact with any reasonable degree of certainty. In consequence of this lack of low-level data, the judgment-makers, both the scientists and the public officials, felt compelled to assume that such changes do occur proportionately, down to the lowest doses, and to make their rules accordingly. This assumption of linear proportionality or linearity is the basis of the present rules for radiation control. Fortunately the most recent work on genetic effects encourages the belief that this assumption introduced a factor of safety, since it has been found that even the larger amounts of radiation necessary for experimental purposes, when delivered at a low rate rather than in one sudden exposure, produce fewer than proportionate numbers of changes.[4] The kinds of human exposure we must deal with involve enormously lower rates of exposure and a thousandfold lower total exposure than those used in the mouse experiments under discussion; hence, it is reasonable to assume that the kinds of biological repair processes that apparently occur in the mouse also work to protect man from the effects of radiation on the genes.

During the period when attention was directed chiefly to genetic effects as a basis for limiting radiation exposure, somatic effects—effects on the body (or soma) of the person exposed—were not ignored. Genetic effects depend on exposure of the germinal cells or the cells from which the germinal cells are formed, through irradiation of the gonads—the ovaries and the testes. Some sources of radiation, however, cause practically no exposure of the gonads while affecting other parts of the body. This condition may arise from exposure to a beam of radiation or through the ingestion and absorption of radioactive materials into the body by way of food, water, or air. The chemical characteristics of such materials are simply those of the elements whose radioactive isotopes are involved. If the body processes cause a particular chemical element to go to one organ and lodge there, that organ will be more heavily exposed than the rest of the body. In some cases, in fact, there may be virtually no exposure of the rest of the body if the localization is nearly total and the kind of radiation emitted by that radionuclide (or radioisotope) is of very short range, such as alpha particles or even beta particles. For such elements—the ones that localize in the body—the carcinogenic effect on the organ of localization is still the governing consideration, and the rules adopted are designed to keep the risk of cancerous changes in that organ acceptably low.

Our knowledge of the carcinogenic effects of radiation has come from a large number of animal experiments and a small number of statistical studies, by epidemiological methods, of chance exposure of humans. Gofman and Tamplin, from a review of the data of those scientists who have studied radiation carcinogenesis in man, have concluded that this risk is greater than previous interpretations have indicated. They contend that the incidence of all kinds of cancer is increased by a constant percentage for each unit of radiation dose absorbed. They hold that this hazard, rather than genetic risk, should be the governing consideration in radiation regulation. Their approach makes no use of the large amount of experimental work with animals but only the few studies of human statistics. This has the advantage of avoiding the undesirable necessity of assuming that effects observed in fruit fly or mouse or dog will occur in similar fashion and to the same extent in man. It has certain disadvantages, however. The investigator must accept the scanty, chance data that exist, with all their limitations of available information, and must try to extract valid relationships from these data by comparison with supposedly similar controls. The selection of controls to make this comparison valid is difficult since the persons chosen must differ in many ways from the exposed subjects and some of these differences

[4] Report of the United Nations Scientific Committee on the Effects of Atomic Radiation, General Assembly Official Records: Seventeenth Session, Suppl. No. 16 (A/5216) (United Nations, New York, 1962), p 92.

may be significant with respect to the effects studied. Ascertainment of dose is invariably difficult and, in some of the studies on which principal reliance has been placed, individual dose determination has been nearly impossible. Another disadvantage is that the exposed subjects in most of these collections of data have received large amounts of radiation, ranging up to 20,000 times the annual doses specified in the FRC guides. In most instances, the doses have been delivered to these subjects at rates of the order of 10 million or more times the rates corresponding to the FRC guides. To draw useful conclusions from data with all these limitations requires some rather extreme assumptions.

BASIS OF CRITICISM OF STANDARDS

Gofman and Tamplin have made such assumptions. They have assumed that doses delivered at the enormously high relative dose rates of the subjects studied produce proportionately the same effects as doses accumulated at the low dose rates of the FRC guides, in spite of the evidence, with respect to genetic dose-effect relationships, that there is marked reduction of effect at lowered dose rates. They point to the absence of studies concerning dose-rate effects on the induction of cancer as justification for ignoring the likelihood that rate has similar effects in genetic and carcinogenic change while at the same time contending that cancer is produced by abnormalities in the genes of the affected cells—that is, by a "genetic" effect in a somatic cell.

In addition to the assumption that the effect of a given small amount of radiation accumulated slowly over a number of years is proportionate to that of a sudden, large dose, they assume that this effect operates as a multiplying factor on the cancer death rates of all ages to the end of life, no matter how long the interval of time since the exposure. Two criticisms must be directed at this hypothesis. The first may be stated, partially paraphrased, in the words of ICRP Publication 14.[5]

". . . the radiation dose required to double the natural cancer incidence is sometimes used in assessing acceptable risks from somatic exposure by analogy with the concept of doubling dose . . . [for] . . . genetic risks . . . This concept of doubling dose for somatic hazards is a specific example of the misuse of the ratio of cancer rates. The natural incidence of [various cancers is so widely variable] . . . depending on the particular population . . . [under consideration that] . . . where acceptable risks and individual varieties of cancer are concerned, the only reasonable parameter to use is the actual number of cases induced by the [given] exposure . . . ." The second point is that most of the studies of radiation carcinogenesis indicate that the cancer-producing effects of radiation fade with time. MacMahon[6] found that "the excess cancer mortality in the X-rayed group [of prenatally exposed children] was most marked at ages 5 through 7 years . . . The excess risk apparently was exhausted by age 8." Court Brown and Doll[7] reported that, although cancers of heavily irradiated tissues in adolescent and adult patients treated for a spinal arthritic disease showed an undiminished relative increase per man-year at risk even in the interval 15–27 years after initial radiation treatment, leukemia and aplastic anemia rose to a peak between 3 and 5 years after first exposure and then began to disappear.

Gofman and Tamplin, nevertheless, having developed a doubtful factor of proportionate increase per unit of radiation (i.e., per rad), apply this factor to the late years of life, when cancer death rates are high, and hence arrive at the large numbers from which they make their estimates of cancer cases attributable to radiation. Even if this hypothesis were sound, which is almost certainly not the case, their figures might be interpreted to mean that people in the late years of life would die about the same time but would succumb to cancer rather than to some other disease. If one must enter this realm of speculation, a much more plausible hypothesis exists. Many investigators have reported a lengthening of the period of latency—the interval between exposure and the appearance of its pathogenic effects—as the dose is decreased. If this effect is extrapolated to very low doses, the latency interval might become 50 years rather than the 5 to 20 years observed at doses hundreds of times greater. The effect, if any, of a small dose accrued to age 30 (an age arbitrarily selected by Gofman and Tamplin) would then be so delayed that few would survive to experience it.

[5] ICRP Publication 14, *Radiosensitivity and Spatial Distribution of Dose* (Pergamon Press, Oxford, 1969), p. 58.
[6] B. MacMahon, *J. Nat. Cancer Inst.* 28, 1173–1191 (1962).
[7] W. M. Court Brown and R. Doll, *Brit. Med. J.* 1965, 2, 1327–1332.

Gofman and Tamplin have been selective in the data they have chosen to use in deriving their multiplying factor. The various studies of human radiation exposure reported in the scientific literature show equivocal results, some of which are contrary to those in the ones they have selected. Moreover, the very studies they cite contain elements which they disregard that tend to contradict their hypothesis. They have, for example, relied heavily on a particular Japanese report[8] based on data accumulated by the Atomic Bomb Casualty Commission (ABCC) on the people of Hiroshima and Nagasaki. The authors of that report have apparently employed some questionable statistical devices that make the few data points seem more orderly than they are in fact. For exmple, although prolonged investigation of the exposure of individuals has made possible only a coarse gradation of estimated doses, relatively fine segregation is plotted in two charts, and the intervals of dose are different in the two. One can only speculate that a different selection of dose categories may have been tried and found to show irregularities as a result of shifting a few of the very small number of cases from one dose range to another. Even in the reported analysis, however, anomalies appear that contradict the hypothesis of Gofman and Tamplin that all forms of cancer are doubled in incidence by roughly the same dose radiation. Certain of the cancers noted in the ABCC study, especially when segregated by sex, actually show decreases with dose, though the extent of decrease is not statistically significant. What *is* significant is that contradictions exist within the studies cited, rendering them poor support for the assumption of *consistent increase*. Such contradictions appear in the case of lung and liver cancers in women, stomach cancers in men, and cancers of the uterus and its cervix.

Gofman and Tamplin have also taken notice of the instances, in the ABCC data and in the data of a British study,[9] in which either no increase or even a slight decrease of effect occurred in going from the lowest dose to the next higher dose, using instead the effects at much higher doses to compute a presumed average effect. When, as in one of the cited studies (see footnote 9), the incidence of cancer in the group irradiated at 1,000 R is the same as in the group given 500 R, the absence of effect from so large an increment of dose seems a better index of what to expect at 0.0005 R per day the inference drawn from cancer incidence at 3,000 R.

A factor that weakens much of the data used by Gofman and Tamplin as compared with experimental data on animals is that most of the human data relates to or includes subjects who are not normal to begin with. The abnormalities may have had no effect on cancer incidence, but it is impossible to rule out that consideration. For example, two of the principal studies used in Gofman and Tamplin's work relate to the incidence of cancer in children X-rayed in the fetal stage.[10,11]

First of all, as MacMahon and Hutchison (see footnote 10) note:

"Ten studies have now been published on [this] question . . . The results of five of these studies support the hypothesis of a carcinogenic hazard . . . and the results of five do not."

These authors then apply certain statistical techniques to argue that the five which support the hypothesis are more reliable than the five that do not. In this review article, however, they make no mention of this factor of possible abnormality of which they are well aware and which is noted in the UNSCEAR report of 1962.[12]

"The [resrospective[13]] studies . . . do not differentiate clearly between the association of leukaemia and (a) the effect of the medical condition which prompted the diagnostic examination, or (b) the effect of X-rays."

The only prospective study[14] showed an insignificantly decreased cancer incidence among those X-rayed, but that study was among the five rejected.

[8] H. Maki, T. Ishimaru, H. Kato, and T. Wakabayashi, *Carcinogenesis in Atomic Bomb Survivors*, Technical Report 24–68, Atomic Bomb Casualty Commission, 1968.
[9] W. M. Court Brown and R. Doll, in *The Hazards to Man of Nuclear and Allied Radiations*, Medical Research Council (Her Majesty's Stationery Office, London, 1956), pp. 87–89.
[10] B. MacMahon and G. B. Hutchison, *Acta, Unio Intern. Contra Cancrum 20*, 1172–1174 (1964).
[11] A. Stewart and G. W. Kneale, *Lancet 1968–I*, 104–107.
[12] Various references cited and summarized in Reference 4, pp. 92–93.
[13] In the retrospective studies, the starting point is a list of victims of childhood leukemia (or other cancer) and inquiry is made as to radiation exposure of the mothers. Prospective studies start with a list of women X-rayed prior to or during a given pregnancy and inquire as to the incidence of leukmia (or other cancer) among these children.
[14] W. M. Court Brown, R. Doll, and A. B. Hill, *Brit. Med. J. 1960, 2*, 1539–1545.

The key point in the hypothesis of Gofman and Tamplin is that 1 rad delivered at any time and at any dose rate produces approximately the same percentage increase in every kind of human cancer per year for the rest of that person's life. Contradictions of this hypothesis as they appear in the human data have already been discussed. It may be well, however, to review and amplify the kinds of uncertainties that affect these data in order to compare them with another source of data not utilized by these authors. Among the sources of uncertainty are: (1) the problem of determining the actual doses received (this is especially troublesome in the case of the Japanese bombing victims); (2) the problem of ruling out other possible causes of increased cancer incidence (as the medical background of women X-rayed during pregnancy or the medical condition of a group of people treated by X-ray therapy for a supposed arthritic condition of the spine); (3) the selection of valid controls, which is extremely difficult in epidemiolotcal studies because the subjects occur by chance and, with the variability of human inheritance and environment, are impossible to match precisely; (4) the effects of heterogeneity of subjects (discussed more fully below); (5) the small numbers of cases of each kind of cancer in the limited groups available for study, so that the accidental variations that normally occur in numbers of deaths from any cause in a given interval can obscure true relationships. Regarding the effects of heterogeneity, its most significant impact on the study may be in the matter of dose uncertainty. If the individuals assigned to a given dose category in the ABCC study, for example, are in fact heterogeneous with respect to dose, it could be that all the observed cases of cancer occur among those who had much higher doses than that attributed to their category. If that be so, the nonoccurrence of cases among those in the lower-dose segment of that category would be obscured and a most significant finding would be lost.

Animal experiments, which Gofman and Tamplin chose not to consider, can be designed to produce the kinds of data desired in the required amounts and with very exact matching of controls with respect not only to environment and care but also to genetic uniformity. Data from such experiments have clearly demonstrated that there are wide differences in the extent to which various kinds of cancers are affected by radiation. Increases are not proportionate to dose in all cases nor does the fractional increase observed in the early part of life after irradiation continue without reduction in the later parts of life.[15]

The most significant parts of the Gofman and Tamplin hypothesis insofar as concerns effect on their numerical results are:

(1) That dose rate and total dose are immaterial in determining the applicability of their hypothesis. It may well be that a 10-millionfold decrease in dose rate—the actual order of magnitude of the exposure differences between the human studies and the FRC guides—wipes out the effect completely or nearly so.

(2) That a carcinogenic effect of radiation observed in a period of 10–20 years after inrradiation continues undiminished for the next 50 years or more. Since cancer is notably a disease of old age, it means that Gofman and Tamplin apply their hypothetical factor of increase due to radiation in exactly the same fashion to the 30-year-olds cancer death rate of about 20 per 100,000 and the 85-year-old's 1,400 per 100,000,[16] using radiation received in infancy as part of the effective dose at age 85. Since 97% of cancer deaths occur above age 30, it is clear that one can generate alarming numbers of hypothetical casualties by the technique described.

The scientific aspects of the issues raised by Gofman and Tamplin do not, then, appear to support their thesis. The human data are at least equivocal, thus undermining the basis of their conclusion, which is the asserted consistency of effects. The animal data, not considered by these authors, show many instances of effects quite contradictory of their hypothesis. In sum, there seems to be no sound reason to depart from the studied conclusion that an average annual radiation dose of 170 millirem presents no serious threat to the health of the individual exposed. It is reasonable, nevertheless, to argue that no unnecessary radiation exposure should be incurred by anyone and especially not by everyone, since the genetic pool of the nation and of mankind may be adversely affected. It is therefore appropriate to consider the regulatory issue and to gauge the effectiveness of the existing system of radiation control.

[15] Various references cited and summarized in Reference 4, pp. 131–134.
[16] Mortality rates estimated from recent U.S. Vital Statistics.

It is here that Gofman and Tamplin, through apparent unfamiliarity with the facts, are led to conclude that control is inadequate and will result in a large population dose, to be multiplied by their proportionality factor. They assume that the FRC guides permit atomic energy activities to deliver to every individual in the nation an annual dose of 170 millirem, and they base their calculations on this dose. In fact, the FRC guides are *not* regulations directed to the users of radiation sources and radioactive materials but rather a statement of guiding principles for the rule-making agencies. In order to abide by that guidance, agencies are compelled by physical laws to conduct their activities in such a way as to insure that such universal exposure cannot occur. (This is discussed more fully below.) To quote the FRC as to the nature of their recommendations (*1*):

(1) They are "for the guidance of Federal agencies in the conduct of their radiation protection activities."

(2) ". . . every effort should be made to encourage the maintenance of radiation doses as far below this guide as practicable." The main Federal rule-making agency, the AEC, applies this principle in its inspection and enforcement practices, as do the state agencies involved in radiation control. The FRC adds: "There can be no single permissible or acceptable level of exposure without regard to the reason for permitting the exposure. It should be general practice to reduce exposure to radiation, and positive effort should be carried out to fulfill the sense of these recommendations." Note use of the phrase, "fulfill the sense," rather than, say, "enforce the guide levels."

(3) "The lack of adequate scientific information [to determine directly radiation effects at low doses and dose rates] makes it urgent that additional research be undertaken and new data developed to provide a firmer basis for evaluating biological risk. Appropriate member agencies of the Federal Radiation Council are sponsoring and encouraging research in these areas." It was the sponsorship, encouragement, and funding provided by the AEC, shortly after the FRC made that comment in 1960, that initiated the project under which Gofman and Tamplin prepared their present publications. The objective was to reach as sound a basis as possible for evaluating risk, presumably through publication of the findings of that project in appropriate scientific journals so that the scientific community—especially the task groups of the ICRP and NCRP—could review the data, analyze and debate the conclusions and, to the extent that seemed wise, make any necessary revisions in the advice on which the Federal agencies base their actions.

(4) In recommending "the use of 0.17 rem for yearly whole-body exposure of average population groups," the FRC adds:

"It is critical that this guide be applied with reason and judgment." As has been pointed out, caution requires the assumption that there is no threshold. If that is true, there will be some harm to unknown individuals and to the nation as a whole as part of the price of a technological development, and any judgment as to acceptable price is somewhat arbitrary. The FRC acknowledges that any balance point of benefit versus risk must be kept flexible and subject to change:

". . . all exposures should be kept as far below any arbitrarily selected levels as practicable . . . any numerical recommendations should be considered as guides."

Since the phrase "as far . . . as practicable" opens the area to the exercise of judgment, one must examine the exposures incurred under the present control processes and how they are likely to be affected by the projected expansion of use of nuclear power. If the FRC guides are found to be inadequate as principles of action for the regulatory agencies, they should certainly be changed. Or if, on the other hand, the regulatory agencies are not maintaining the necessary control to insure that we are living within the guidelines, that control should be tightened. First, however, it is necessary to get the facts. Gofman and Tamplin acknowledge that the people of the United States are not getting 0.17 rem per year from current atomic energy activities, but they predict that everyone is likely to get such doses in the future under the present control system because the reference points designated by the FRC as the "Radiation Protection Guides" are equivalent to that average dose.

The scientists' recommendations from which the FRC derived the figure, 0.17 rem, were that no individual in the general population should get more than 0.5 rem per year (to insure that each individual's personal risk is small) and that the accumulated gonadal or genetic dose to the national population pool should average no more than 5 rem in 30 years (age 30 was chosen since it is approxi-

mately the median age of parents at the time of birth of their children). The latter criterion—the genetic one—represents an average of 0.17 rem per year. At the same time, a technique for making it reasonably certain that no one exceeds the 0.5-rem somatic criterion, even though we cannot keep track of each individual's exposure, is to limit the average of a representative sample to one third that amount—again, 0.17 rem per year. So the two criteria roughly coincide in practice.

The amounts of external radiation that may be generated and the amounts of radioactive materials that a user may discharge into the environment are then limited by regulations so that no one, even if he spends every hour of his life at the site boundary of a nuclear plant, is likely to exceed the 0.5-rem ceiling as a result of the plant operations. For that purpose, limits are set on the concentrations of individual radionuclides that are permitted in the air or in the surrounding waters. Now, to comply with this requirement, a plant must be designed and constructed with a margin of safety that aims at a tighter control than the minimum. Then, in operation, the need to insure that some incident later in the year will not bring the total discharge above the allowable amount makes it necessary for the plant operator to maintain the lowest practicable discharge of radioactive effluents at all times. The net effect is that even the most-exposed individual is unlikely to get the maximum exposure legally permitted. For everyone else, additional factors of safety are introduced by natural laws. Since the most-exposed individual must be in the downwind direction and air concentrations of radionuclides must be held within limits for him, all other directions will receive lower levels. Diffusion of effluents with distance from the plant dilutes the concentration still further.

The practical answer to the question whether the FRC guide level will be treated as both floor and ceiling for population exposure should be sought in the data that have been collected and reported[17] regularly from a wide area around each existing nuclear power plant. The experience here gives a clue, at least, to whether nuclear power is pushing its limits locally. These data include not only the air and stream concentrations but also, because of concern about selective absorption and retention of certain nuclides in living systems, the concentrations in vegetable and animal life in nearby lands and waters. With only rare exceptions, the data show no detectible increase in the radioactivity present in the environment when a nuclear power plant begins and continues operation. This is not surprising when it is realized that the design basis for a typical reactor is to allow no more than 5 millirem per year at the site boundary rather than the guide level of 500 and no more than 0.1 millirem per year in what is known as the "low-population zone" surrounding the location. Such values are too low to measure directly but can be estimated by calculation.

Because of the refined measuring instruments available, it has been possible in some locations to detect radionuclides concentrated through various living organisms, a so-called "food chain." Such cases are not to be confused, however, with the much-publicized condition that occurred some years ago in the vicinity of the reactors at Hanford, Washington. There, 8 large reactors were being operated to produce plutonium, and the heat they produced was being intentionally wasted—a situation that was corrected several years ago. To dispose of the waste heat, Columbia River water was run into the reactors, where it absorbed heat and became radioactive, and was then run back into the river. The purpose of power reactors, on the other hand, is to use the heat to generate electricity, so they operate in various types of closed cycles that bear no resemblance to the former Hanford operation. Most current designs use water to transfer the heat to a turbine, and many retain the water in a fully closed cycle. Those that convert the reactor cooling water directly to steam do extract the noncondensible gases from the condenser and discharge them into the atmosphere, but these contribute almost nothing to radiation exposure. No concentration of radionuclides through food chains as a result of reactor operations has been of sufficient magnitude to produce measurable exposures to man, so far as an extensive search of reports has revealed. The mandatory reporting system assures that, if any significant exposure had occurred, it would have been well publicized.

The radionuclides produced in the fission process represent huge quantities of radioactivity, but while they are in the nuclear fuel at the power plant, the risk

[17] Among many sources, one of the most important is the monthly publication, *Radiological Health Data and Reports*, U.S. Public Health Service, which began in April 1960 as *Radiological Health Data*. It is available at $6.00 a year from the Superintendent of Documents, Washington, D.C. 20402.

of escape of significant amounts is neglible. At the fuel reprocessing plant, more difficult problems arise, since this highly radioactive material must be removed from its containers and chemically processed. Only one such plant is in commercial operation in this country; in addition, AEC reprocesses fuel at three contractor-operated plants. It may be some time before there is another commercial plant, and there will be no need for many of them even decades from now, since each reprocessing plant can handle the fuel from many reactors. The sole operational plant handling the fuel reprocessing for nuclear power stations is located in a carefully chosen, isolated site within a 3,300-acre industrial park in New York that was acquired by the State for atomic energy development purposes. Such plants will require the most stringent controls for protection of the local environment but need not be weighed as major contributors to the exposure of the entire nation. In short, all the evidence demonstrates that, far from an average dose of 170 millirem per year to persons in the immediate vicinity of nuclear plants, the present regulatory control has held their average exposure to undetectably low levels. Even a network of nuclear power plants such as might exist decades hence presents no prospect, therefore, of an environment contaminated to the level of the FRC guides. To assume that practices and control procedures will change drastically for the worse is to single out for criticism one of the rare fields of industrial activity that has taken anticipatory precautions rather than lagged in applying safety measures.

A copy of a later presentation [18] by Tamplin at the annual meeting of the American Association for the Advancement of Science in Boston, December, 1969, came to hand too late for detailed analysis in this article. Brief examination of a draft copy shows that, in the same vein as the Gofman and Tamplin papers discussed herein, it postulates inconceivable situations from which to draw alarming inferences. There has been insufficient time to review this paper critically, but its shortcomings are evident in the first few pages, where the author presents the following example:

"I will illustrate this by making use of a hypothetical river . . . some 200 km long, that is a little over 100 miles. . . . Now actually this hypothetical river is not an unreasonable river in terms of many rivers for which reactors are planned . . . I assume that the population exists totally on a diet of aquatic origin that is derived from this river . . . [There follow some pages of calculation of the effects of an assumed discharge of about 0.01 to 0.06% of the fission products produced in a year in a 1,000-MWe reactor.] . . . The other point of importance in this respect is, for a river on which a number of reactors and one or more fuel reprocessing plants are planned, one has to consider . . . [the additive effects]."

A 1,000-MWe plant can take care of the entire electrical needs of a population of about 800,000. Since projections of nuclear power development show that fossil fuel plants are likely to account for about 80% of the total generating capacity in the year 1980, it may be assumed that 4,000 MWe of fossil-fueled power plants exist in the same vicinity, servicing a population of 4 million. If "a number" of such nuclear stations are imagined to exist on a 100-mile stretch of river, there is a population of tens of millions that, in some miraculous fashion "exists totally on a diet" derived from that river! Then, too, the assumption that some hundredths of a percent of the fission products get out of the closed system, with its multiple layers of protection, and into the river in the course of reactor operations is so far from the realities of reactor experience that discussion is pointless. In sum, here again unrealistic assumptions are used to create a hypothetical hazard that does not in fact exist.

Gofman and Tamplin have responded in the following fashion to critics who have pointed out the well-documented, though probably underestimated, cost in life and health from the use of gas, oil, and coal instead of nuclear energy to produce power and heat:

"We don't condone homicide with knives any more than homicide with guns. ". . . If fossil-fuel plants are causing disease . . . [they] should be redesigned to remove effluents that are producing harm.

". . . It is much better to pay a little more for electricity [because of increased radiation protection costs] than to die prematurely of cancer or leukemia."

In the real world, however, the choice is immediate and the facts on which to base it are at hand. Even though the technology of using fossil fuel in power

[18] Discussion is based on a draft copy of: A. R. Tamplin, ***Nuclear Reactors and the Public Health and Safety.***

plants, industries, homes, and vehicles has been greatly improved through the years, there is no prospect whatever that the hidden costs in life and health involved in the production, transportation, and utilization of fossil fuels can be brought down, now or in the near future or possibly ever, to the same low level as is entailed in the generation of power by use of nuclear energy.

CONCLUSION

It is logical right now, on public health grounds, to prefer nuclear power. Although the nuclear power industry is apparently meeting, in practice, the tenfold more stringent FRC guides that Gofman and Tamplin propose, to make such lower levels a binding requirement would mean that designers, builders, and operators would again have to inject a factor of safety that would make reactors uneconomical and would force utilities to choose fossil fuel. Mankind has lived with fossil fuel for a long time and has accepted its risks along with the risks that every activity of our lives introduces. Nuclear power, a newcomer in the field, promises to be one of the least risk-laden developments of technology.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION, LAWRENCE RADIATION LABORATORY,
Livermore, Calif., February 19, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: Recently Dr. Alexander Grendon of the Donner Laboratory submitted to you a document entitled "Inferences From Radium Exposure In Humans".

Were it only to discuss the substantive matters of that document, we would not even take the time to comment at all, for we find no substance whatever in the Grendon document. Nor would we be the least concerned about Dr. Grendon's frivolous and insulting remarks about our efforts. The reader of the Grendon document will easily be able to handle that evaluation, without help from us.

However, there are certain major issues raised by what Dr. Grendon has done that are so shocking to us that we must raise them for inclusion in the Hearings.

In his document, Grendon states, (Quote Grendon) "It is unfortunate that Gofman and Tamplin do not represent Evans' hypothesis on the basis of his published papers but rather on the basis of some remarks he made in oral testimony before a Congressional committee".

When we saw this statement, we were surprised beyond belief. The hypothesis of Evans, which we tested in Radium I (1), and Radium II (2), are from *published* documents of Evans in no less than 5 sources, a listing of which is appended to this letter (3), (4), (5), (6), (7). Since we couldn't believe what we read in the Grendon quotation, we asked Dr. Grendon how he could possibly be so confused. His reply was that he (Grendon) didn't happen to have a copy of our first paper (Radium I) available when he wrote his critique, and, so, thinking he knew what it might have contained, he just went ahead to criticize us without bothering to read what we had said. We had always been under the impression that when one writes a critique of someone else's work, the first step is to *read* that work.

Professor Lawrence, the Director of Donner Laboratory has assured us (see his letter attached) that he has great faith in Dr. Grendon's integrity and that he feels sure Dr. Grendon will now go back and *read* our article and then revise his manuscript in a manner reflecting that he now knows what we were writing. We hope so.

Dr. Grendon, in his critique, states an Evans' hypothesis that he (Grendon) thinks we should have addressed. Let us quote Grendon again. (Grendon) "(1) They do not give proper consideration to Evans' hypothesis about the relation of latency to dose".

With respect to any *reasonable* relation of latency to dose, our paper clearly gives consideration to this issue. With respect to Grendon's quotation of Evans' latency periods of 45 to 250 years, we have the following to say.

In a forthcoming paper on this subject (8), we point out that the fallacious handling of the latency question by Evans is even more incredible than the hypothesis of Evans that "the linear, non-threshold model is incorrect".

Although the list of Evans' documents stating the hypothesis we tested is abundant (see enclosures), we must comment on a new philosophy introduced by the Grendon document. While he is incorrect in stating that our papers are based upon "oral testimony before a Congressional committee", the implications of the Grendon statement are indeed astonishing. According to such implications, one need never fear being held responsible for oral statements made before a Congressional Committee—statements that the author always has an opportunity to check for minor errors they are printed in Hearings documents.

Were you aware that witnesses might take the view that they could say anything they choose before your Committee and then expect the content and impressions to be expunged from the record at will, with no responsibility for them? This *must* be a new concept to all of us.

Sincerely yours,

ARTHUR R. TAMPLIN.
JOHN W. GOFMAN.

Enclosures: (1) Professor Lawrence's letter explaining Grendon's actions.
(2) A list of the Evans' *published* (*not* oral) documents in which the Evans' hypothesis concerning "linear non-threshold models of carcinogenesis" is presented.
(3) The references to our previous Radium I and Radium II papers and the forthcoming one to be submitted to the journal "Health Physics".

[Enclosure 1]

UNIVERSITY OF CALIFORNIA,
DONNER LABORATORY AND DONNER PAVILION,
Berkeley, Calif., February 11, 1970.

Prof. JOHN W. GOFMAN,
Biomedical Division,
Lawrence Radiation Laboratory,
Livermore.

DEAR JOHN: Thanks for your letter of February fifth. I know Alex Grendon well enough to have tremendous confidence in his integrity so that he will restate or rewrite his paper or make changes when the proof comes after reading your first radium therapy paper, which he must not have access to. That's the only explanation that I have.

With reference to the Budinger paper, it was my understanding that he attempted a summary of the world literature on this subject, and Dr. Totter got such summaries from several laboratories, and it was my understanding that this was not to be published. Tom Budinger doesn't feel that his material is in shape yet for publication. I certainly know Ernest's views on matters ethical and scientific, and being close brothers he and I were in full agreement on such matters.

With warm personal regards,

Sincerely.

JOHN H. LAWRENCE, M.D.

[Enclosure 2 and 3]

FEBRUARY 19, 1970.

REFERENCES

1. Gofman, J. W. and Tamplin, A. R. "Studies of Radium-Exposed Humans: The Fallacy Underlying a Major Foundation of NCRP, ICRP, and AEC Gidelines for Radiation Exposure to the Population-at-Large". Supplement to Testimony presented before the Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, 91st Congress, November 18, 1969.
2. Gofman, J. W. and Tamplin, A.R. "Studies of Radium-Exposed Humans II: Further Refutation of the R.D. Evans' Claim That The Linear, Non-Threshold Model of Human Radiation Carcinogenesis is Incorrect". Supplementary Testimony. ibid and JCAE Hearings, January 28, 1970.

LISTED BELOW ARE THE REFERENCES OF EVANS THAT APPARENTLY ESCAPED DR. GRENDON'S NOTICE

3. Evans, R. D. "The Effect of Skeletaly Deposited Alpha Emitters in Man". *Brit. J. Radiol. 39*, 881–895, 1966.

4. Evans, R. D. "The Radium Standard for Boneseekers—Evaluation of the Data of Radium Patients and Dial Painters". *Health Physics 13*, 267–278, 1967.
5. Evans, R. D., Keane, A. T., Kolenkow, R. J., Neal, W. R., and Shanahan, M. M. "Radiogenic Tumors in the Radium and Mesothorium Cases Studied at MIT" pp. 157–194 in "Delayed Effects of Bone-Seeking Radionuclides", edited by Mays, C. W. et al., University of Utah Press, Salt Lake City, Utah, 1969.
6. Evans, R. D. "On the Carcinogenicity of Inhaled Radon Decay Products in Man" (CORD), July 1967, in "Radiation Exposure of Uranium Miners" Appendix *18*, pp 1188–1207, Hearings of the Joint Committee on Atomic Energy Part 2, May 9 through August 10, 1967.
7. Evans, R. D. (*Written* presentation, *not* oral) "Round Table Discussion Topics for the July 26, 1967 JCAE Hearings on the Uranium Miners". Appendix *19*, ibid, pp 1213–1216.
8. Gofman, J. W. and Tamplin, A. R. "The Question of Safe Radiation Thresholds for Exposure to Ionizing Radiation in Humans". To be submitted February 25, 1970 to "Health Physics".

Senator MUSKIE. Thank you very much, Dr. Gofman, for your very lucid and helpful statement. Even a layman like myself understands what you are saying.

May I ask with reference to your concluding comments have you submitted these same conclusions and same evidence to the AEC? If so, what if any reaction have you obtained?

Mr. GOFMAN. We presented these comments basically, both personally to members of the AEC and in a presentation before the Institute of Electrical and Electronic Engineers on October 29 and we sent copies to the Atomic Energy Commission. I believe it is fair to say that they are digesting them.

(See appendix II, p. 286, for comments.)

Senator MUSKIE. There is no radioactive risk associated with digesting them.

Mr. GOFMAN. At the moment, not yet.

Senator MUSKIE. Have you submitted this same evidence to the Joint Committee on Atomic Energy?

Mr. GOFMAN. Yes; they have copies.

Senator MUSKIE. You referred to a handbook which has been produced by Dr. Tamplin.

Mr. GOFMAN. Yes, that handbook is here. It is available together with the supporting documents. That handbook is in looseleaf form so that every time new data become available it can be updated. It can be used for Plowshare events, it can be used for reactors, or any type of release. It enables one to go from the source term of what nuclide is released to what that nuclide will give in dosage to a particular organ of man.

That handbook is at times criticized because it is restrictive and that is because it is a "worst case" estimate. When we don't have enough data on something, or don't know what a number is, we use the most conservative number.

Senator MUSKIE. I don't quarrel with that standard at all. From the size of that the description "handbook" might be misleading. I take it that is available to us for our files?

Mr. TAMPLIN. Yes, it is. We brought it to give to the committee.

Senator MUSKIE. We appreciate that.

Mr. TAMPLIN. It is going to be considerably larger. When we add the next addition to it we are going to put in a set of tables that apply directly to release from nuclear reactors. At this time tables are most pertinent to Plowshare activities.

HUMAN EFFECTS

Senator MUSKIE. Your studies have been limited to effects upon humans. Have you gone beyond that at all?

Mr. GOFMAN. There is a vast body of evidence, developed largely under the auspices of the AEC concerning a variety of animal species. We are personally not involved in such studies. There is a vast literature on it. I feel that we should use human data when we do have them. It is unfortunate that we have them from Hiroshima-Nagasaki, it is unfortunate that we have them from the uranium miners, it is unfortunate that we have them from the irradiation of patients from nonmalignant diseases, but we have them. They provide answers.

We are delighted that the AEC does such a marvelous job in sponsoring animal research throughout the country which bears upon these questions. We are personally not engaged at this time in animal studies but there are extensive animal studies in our laboratory concerning many other problems.

Senator MUSKIE. Risk to humans and other members of the ecosystem?

Mr. TAMPLIN. Yes.

Senator MUSKIE. Your estimates here I take it don't take those risks into account?

Mr. TAMPLIN. We can and we do estimate the dosage that would occur to other members of the ecosystem. So far as estimating what the effects on such ecosystems would be, we haven't gone into that, although there is extensive literature, but I think when one gets through reviewing it in detail that you would have to conclude that the evidence isn't sufficient to answer the question of the long-range consequences of low-dosage radiation delivered to sizable portions of an ecosystem.

So I think the data, so far as the ecology goes, will be less solid than the data that we have available on man.

FOOD CHAIN EFFECTS

Senator MUSKIE. Do you feel entry of these risks into a food chain measurably increases the risk to humans?

Mr. TAMPLIN. I am sorry.

Mr. GOFMAN. I think he was asking about the food chain. One thing we would like to mention. This handbook has every dose to humans estimated including the food chains. We do not consider secondary standards of maximum permissible doses in air and water as an answer because they often don't include the food chains. This handbook does in every case include food chains to man. Isn't that correct?

Mr. TAMPLIN. Yes. As a matter of fact as part of the conservatism we suggest that when you are looking at the dosage that might be derived from radionuclides introduced into water, that you calculate the dosage for individuals existing totally on a diet of aquatic origin. That means they are eating fish and bread made from the algae and they are eating plants from aquatic sources.

So if one calculates it that way, then one is being the most critical, you are looking at the problem the most critical way, and we do include the concentration of the radionuclide from the water into the biological material in the calculations.

Senator MUSKIE. Is there adequate study underway to examine the various ways in which radioactivity is dispersed once it is released into the environment through the atmosphere, through water, and so on?

Mr. TAMPLIN. There are a number of studies underway and in treating the introduction of radionuclides into the aquatic systems we have a model that can be either conservative or you can make it less conservative. So far as the introduction of radionuclides into the atmosphere, say from a Plowshare explosion, we suggest in terms of protecting public health and safety that one has to assume a worst case situation for fallout of that in the atmosphere, and this represents a situation where the cloud runs into a rain storm and is immediately rained out below the cloud.

And in looking at the data that came up from the weapons testing programs of the past, we find that a number of situations have occurred where the actual fallout level was within a factor of 10 of this "worst case" estimate. So we feel that one has to use the worst case approach.

RISK OF RADIATION EFFECTS

Senator MUSKIE. Do you have any judgment as to whether or not the Plowshare program, once it becomes operational, will involve a level of risk comparable to that in the weapons testing program under the limited test ban treaty?

Mr. TAMPLIN. It would be in a very local area. The Plowshare explosives are much cleaner than the explosives that were used in the weapons programs. The amount of radioactivity released into the atmosphere is substantially reduced. We have estimates in the handbook and related documents for the dosage that might be delivered to people from the construction of a nuclear canal in Panama, and on the basis of these estimates it is possible to feed this back into the device design so that Plowshare device designers can use these estimates to redesign their device so they would produce minimal radiation dosage to individuals.

Senator MUSKIE. I note that Dr. Gofman in his concluding pages indicated optimism with respect to the capacity of our engineers to reduce the risk of radiation in the Plowshare program if we but set the standards that you suggest.

Mr. GOFMAN. Senator Muskie, I think I would modify that a little bit When you say I have very great confidence that our engineers can design powerplants, reactors, reprocessing plants, and perhaps storage facilities in such a way as to really work toward absolute containment, that is true. I don't have that kind of confidence that the excavation type experiments by Plowshare can ever reach that point, and I have extremely grave reservations as to whether excavation projects make any sense at all.

Any project that has by design the spewing out of radioactivity into an uncontrolled region doesn't make sense to me, simply as I view it today. I have given it great consideration and have worked in the Lawrence laboratory as an associate director of that laboratory, which is concerned with Plowshare and other problems, for 6 years.

My summary impression is that at the present moment with the fact that the evidence leads us to worry about reducing standards a

factor of 10 rather than raising them, any projects devoted toward the indiscriminate or discriminate spewing of radioactivity into the environment are very hard to make sense out of.

Senator MUSKIE. I must say at this point that is my reaction as a layman. I guess I will wait for further evidence before forming judgment.

Mr. TAMPLIN. I think the other thing is that if the allowable radiation dosage to the population is reduced substantially as the data would indicate that it should be, that Plowshare would probably not be able to meet this standard, at least for a number of years off in the future.

So you can protect the public from Plowshare excavation programs by lowering the standards, and therefore, Plowshare won't be able to conduct their excavation program.

Senator MUSKIE. It is implicit, isn't it that standards of the kind we discussed this morning are necessary, that any discharge of nuclear devices must be controlled?

Mr. GOFMAN. Very severely.

Senator MUSKIE. To the extent that it is not controllable it ought not to be permitted.

Mr. GOFMAN. I would agree with that.

Senator MUSKIE. I would like to read just a couple of quotations that I would like you to comment on.

According to testimony by Dr. Paul Tompkins, Executive Director, Federal Radiation Council, on November 6 of this year he stated:

> Increasing knowledge over the past 15 years indicates that the radiation dose prescribed by the FRC is probably less rather than more hazardous to health and well-being than was thought at the time.

Mr. GOFMAN. Is that a statement that even that dose would be less hazardous?

Senator MUSKIE. He said it was probably less rather than more hazardous.

Mr. GOFMAN. I would be delighted to see any of the evidence, I mean evidence concerning humans, on which Dr. Tompkins can make that statement because from the evidence I have looked at, I feel his statement stands in direct contradiction to everything I considered here today.

Senator MUSKIE. We will provide you with a complete statement, and any comments you would like to submit, we would like to have.

The other statement predated that by several years. On June 29, 1965, Dr. Tompkins stated:

> The numerical values for these guides were placed as close to the annual dose from natural background radiation as technical, economic, and operational considerations in the nuclear industry allowed.

Could you discuss any of the assumptions of that statement in your testimony this morning? That is a very strong assumption that natural background radiation isn't a problem, isn't it?

Mr. GOFMAN. If he is assuming natural background isn't a problem, I think that is the first error. I have pointed out to you I think natural background causes about 3 percent of the spontaneous forms of cancer. By no means would I consider that a problem of the order of 3 percent of the spontaneous incidence of cancer is small. If we as a society should go about adding a hazard equal to that, that represents reverse

public health equivalent to all the advances of public health that have been made in the past 25 years.

Mr. TAMPLIN. I think another interesting point about that guideline is the currently estimated dosage, genetically significant dosage derived from medical X-rays, is about 1½ rads in 30 years. That is a factor of three below the FRC guidelines. I think we all know the real benefits that are derived from medical radiation, so it is rather strange that the peaceful uses of nuclear explosives or other peaceful applications should be allowed to exceed it.

In addition to that, the medical profession is making concerted efforts to drastically reduce that amount, so it probably means the medical profession is going to try to deliver something that is more than a factor of 10 below what people could get, exposed from nuclear energy. Medicine is making every effort to drastically reduce that dosage.

Mr. GOFMAN. There is a good reason for optimism that that number can and should drop by a factor of 10 to 100 in the future from the medical exposure. But you can't do that if you spew radioactivity out by industrial or Plowshare applications.

Senator MUSKIE. I would like to read something else from Dr. Tompkins' statement of 1965. Something he said here that I think ought to be in the record.

> Our general conclusion was that the likelihood is relatively small that environmental contamination levels, either from past testing or from the kind of testing that one would anticipate as likely over the next several years, will significantly exceed the established guidelines which the Council provided earlier for the control of normal peacetime operations.
>
> Accepting the reasoning that a level such as that specified for the control of normal peacetime operations is acceptable to gain the benefits offered by nuclear industry, one cannot justify initiating protective actions for fallout at lower levels.

That seems a rather incredible policy assumption in the light of your testimony here this morning.

Mr. GOFMAN. It was incredible to me then and it is incredible to me today.

Senator MUSKIE. I think for the record I ought to note that the Federal Radiation Council was formed by Executive order in 1959. Perhaps there is time to review this mechanism for protecting public health. That Executive order without objection will be included in the record at this point.

(The information to be furnished follows:)

EXECUTIVE ORDER No. 10831

[Aug. 17, 1959, 24 F.R. 6669]

ESTABLISHMENT OF FEDERAL RADIATION COUNCIL

By virtue of the authority vested in me as President of the United States, it is hereby ordered as follows:

Section 1. (a) There is hereby established the Federal Radiation Council (hereinafter referred to as the "Council").

(b) The Council shall be composed of the Secretary of Defense, the Secretary of Commerce, the Secretary of Health, Education, and Welfare, and the Chairman of the Atomic Energy Commission.

(c) The Chairman of the Council shall be designated by the President, from time to time, from among the members of the Council.

SEC. 2. The Council shall advise the President with respect to radiation matters directly or indirectly affecting health, including matters pertinent to the general guidance of executive agencies by the President with respect to the development by such agencies of criteria for the protection of humans against ionizing radiation applicable to the affairs of the respective agencies. The Council shall take steps designed to further the interagency coordination of measures for protecting humans against ionizing radiation.

SEC. 3. The Special Assistant to the President for Science and Technology, or his representative, is authorized to attend meetings of, to participate in the deliberations of, and to advise with, the Council.

SEC. 4. For the purpose of effectuating this order, each executive agency represented on the Council shall furnish necessary assistance to the Council, in consonance with section 214 of the act of May 3, 1945, 59 Stat. 134 [31 U.S.C.A. § 691]. Such assistance may include detailing employees to the Council to perform such duties consistent with the purposes of this order as the Chairman of the Council may assign to them. Upon the request of the Chairman of the Council, the heads of executive agencies shall so far as practicable provide the Council information and reports relating to matters within the cognizance of the Council.

SEC. 5. The Council may seek technical advice, in respect of its functions from any source it deems appropriate.

DWIGHT D. EISENHOWER.

Senator MUSKIE. May I add that the subcommittee will submit your testimony this morning to the Council inviting its comments. And we may very well submit those comments to you for such additional analysis as you would like to give.

Senator MUSKIE. Gentlemen, I am most grateful for your testimony this morning. You have added to my education. And for serious students and others skilled in the art, I will place in the record all the attachments that accompanied your statement. The references, I am sure, will be of great benefit to those reading this record. If you do respond to the comments of the Council, your letter will appear also.

(Subsequent to the hearings agency comments were solicited in the following letters. Responses and supplementary statements by the authors appear as an appendix II to this volume.)

JANUARY 23, 1970.

Dr. JOHN W. GOFMAN,
Professor of Medical Physics, University of California,
Berkeley, Calif.

DEAR DR. GOFMAN: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass over any of the attached which are outside your area of specialty or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(The following letter was sent to the names listed below:)

Hon. Clifford M. Hardin, Secretary, Department of Agriculture;
Hon. Glenn T. Seaborg, Chairman, Atomic Energy Commission;
Hon. Maurice H. Stans, Secretary, Department of Commerce;
Hon. Melvin R. Laird, Secretary, Department of Defense;
Dr. Paul Tompkins, Executive Director, Federal Radiation Council;
Hon. Robert H. Finch, Secretary, Department of Health, Education, and Welfare;
Hon. George P. Shultz, Secretary, Department of Labor;
Dr. F. D. Sowby, Scientific Secretary, International Commission on Radiation Protection, Sutton, Surrey, England; and National Council on Radiation Protection and Measurements, Washington, D.C.

DECEMBER 1, 1969.

DEAR ——: Recently the Subcommittee on Air and Water Pollution began hearings on the potential environmental effects associated with underground uses of nuclear energy for excavation and other purposes.

These hearings focused on the present level of understanding of the environmental implications of this technology for canal construction, gas field development, copper leaching, and other purposes. Testimony was received from distinguished experts with experience in the ecological and environmental health effects of radioactive contamination of the environment.

Of particular interest was the statement presented by Dr. John W. Gofman and Dr. Arthur R. Tamplin, a copy of which is enclosed.

Drs. Gofman and Tamplin opened their testimony as follows:

"We wish to apprise you that, in our opinion, the most crucial pressing problem facing everyone concerned with any and all burgeoning atomic energy activities is to secure the earliest possible revision *downward*, by *at least a factor of tenfold*, of the allowable radiation dosage to the population from peaceful atomic energy activities. The Federal Radiation Council allowable dose of whole body ionizing radiation is 0.17 Rads per year. We shall present to you hard evidence that leads us to recommend that this be reduced now to 0.017 Rads or even less."

This statement was followed by an impressive analysis of the need for the suggested revision. Because of the nature of this testimony and the credentials of the witnesses I would appreciate your comments and evaluation of this recommendation.

Your early response to this inquiry will be greatly appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(Whereupon, at 12:15 p.m., the subcommittee recessed, to reconvene at 9:30 a.m., Wednesday, November 19, 1969.)

UNDERGROUND USES OF NUCLEAR ENERGY

WEDNESDAY, NOVEMBER 19, 1969

U.S. SENATE,
SUBCOMMITTEE ON AIR AND WATER POLLUTION
OF THE COMMITTEE ON PUBLIC WORKS,
Washington, D.C.

The Subcommittee on Air and Water Pollution met at 9:40 a.m., pursuant to recess, in room 4200, New Senate Office Building, Senator Edmund S. Muskie (chairman of the subcommittee) presiding.

Present: Senators Muskie and Gravel.

Also present: Richard B. Royce, chief clerk and staff director; Mr. Barry Meyer, counsel; Leon G. Billings, Richard D. Grundy, professional staff members; Tom C. Jorling, minority counsel, and Walter Planet, Department of Commerce Fellow.

Senator MUSKIE. The committee will be in order.

We have two more distinguished witnesses this morning. We are privileged to welcome first of all Dr. Edward Radford, professor of environmental medicine, Johns Hopkins University. Dr. Radford, it is a pleasure to welcome you, and I express my appreciation for your testimony this morning.

STATEMENT OF EDWARD P. RADFORD, M.D., PROFESSOR OF ENVIRONMENTAL MEDICINE, THE JOHNS HOPKINS UNIVERSITY SCHOOL OF HYGIENE AND PUBLIC HEALTH

Dr. RADFORD. Thank you, Mr. Chairman. I could proceed by reading the document that I have presented before the committee in toto, or we could dispense with the reading of it—however you would like to proceed.

Senator MUSKIE. I haven't had an opportunity to read it in advance, so you want to make sure to cover it fully.

Dr. RADFORD. All right. I will proceed, then. I am sorry I didn't have an opportunity to get it to your committee before this.

Senator MUSKIE. No harm done at all.

Dr. RADFORD. I have been asked to appear before the subcommittee to discuss the public health aspects of underground uses of nuclear energy for excavation and other purposes. My qualifications include medical training, many years' experience teaching public health subjects, particularly radiation biology and toxicology, and field experience related to nuclear bomb tests at Eniwetok and Kwajulein atolls during the test series in 1948.

During the past year, I have served as Chairman of the Sub-Task Force on Physical Factors of the National Institute of Environmental

Health Sciences Task Force on Research Planning in Environmental Health Sciences.

My experience with the atomic energy program has not included a close association during the period of underground testing of nuclear weapons nor of the Operation Plowshare program. I am therefore familiar only in a general way with the results of environmental monitoring done after underground detonations. I shall base my remarks concerning the public health implications of underground uses of nuclear energy on reasonable suppositions concerning the release of radioactive materials to the environment.

It has been my understanding that the proposed use of nuclear explosions for engineering purposes is not restricted to underground detonations but may include surface explosions. Because S. 3042 specifically is concerned with environmental effects of underground explosions, however, I shall restrict my discussion to them.

It is perhaps worth emphasizing that public health problems of surface or air detonations probably would be much greater than those arising from underground uses. As in the case of weapons testing, restriction of explosions to underground locations presumably has been made because of local and worldwide health implications.

I shall not discuss such matters as seismic effects of detonations, since they are outside my field of competence. Other effects of underground explosions may be present, some of which may have health implications. For example, there could be release of naturally occurring minerals or gases into the environment, with effects on living organisms nearby. Such effects will depend on the particular location of the detonation and, at least in part, may be predicted in advance and incorporated into advance planning to protect individuals who may be affected.

The health effects principally of concern are those associated with radioactivity which may be released to the environment from the explosion. These arise from three sources: (*a*) unchanged nuclear fuel, generally isotopes of uranium or plutonium for fission reactions; (*b*) products of the energy-releasing reaction, notably fission products in the case of conventional heavy element fission; and (*c*) activation products, or radioactive materials produced from elements near the explosion, and which are initially not radioactive.

For an explosion with natural uranium-238 as fuel, plutonium is an activation product of unfissioned fuel. In the case of thermonuclear explosions, the most important material released is tritium, the heavy radioactive isotope of hydrogen, which may occur because of all three of the above reasons.

The physical form in which these materials will be released depends on their physico-chemical properties. Some materials, notably isotopes of iodine, xenon, and krypton, will be gases or vapors which may easily escape through vents or fissures to the ground. Others, such as plutonium, may be molten at the temperature of the explosion, cooling into small spheres which are highly radioactive but which do not move readily from where they are deposited.

Most of the radioactivity will exist initially as a dust, or fine particles, some of which can be readily dissolved in water and reach the surface in this way. It is possible that the radioactive atoms will bind

physico-chemically to rock surfaces in the explosion cavity, and thus not be easily transferred to other points.

The particular health concerns relate to long-lasting effects from low levels of exposure. Exposures may occur over a short period of time, as was the case for iodine-131 absorption into the thyroid gland by the Marshall Islanders exposed from weapons-test fallout, or over long periods of time if long-lived radionuclides are distributed widely from the explosion site. In the latter case, the materials may be transported long distances, and thus come in contact with large populations.

Health effects of this kind include genetic changes in the germ cells of the ovaries and testes, and possible cancer production in many of the kinds of cells found throughout the body. In considering the potential implications of underground explosions, it is useful to separate the kinds of energy sources used for this purpose into two categories: Fission reactions and fusion, or thermonuclear reactions.

In the former, heavy nuclei are split in two or three pieces, while in the latter, two light nuclei—deuterium and tritium—are made to combine into a heavier nucleus, helium. In both types of reactions, a large amount of the energy bound in the reacting nuclei is released. Obviously, however, the products will be greatly different in the two cases.

FISSION DETONATIONS

The radionuclides of particular environmental importance from fission explosions are now well known. With the discovery that tritium is a fission product in 1959, it appears that our knowledge of the significant radioactive materials released from a fission detonation is complete. So far as the environmental movement and public health implications of these materials is concerned, a number of general characteristics of the different radioisotopes can be defined. These include the following:

1. Probability of rapid release to the environment at the time of the explosion. For small releases, the amount escaping through cracks or fissures will depend on the volatility of the particular element or its compounds.
2. Physical half-life (the time it takes for half of the atoms to decay) which will determine the time available for the isotope to move away from the site.
3. External radiation doses possible from the radio-nuclides either released directly to the atmosphere or reaching the surface by other means. This property is determined by the extent of penetration of the radiation emitted.
4. Likelihood of binding to inert materials either in the cavity produced by the detonation or in surface materials.
5. Solubility or incorporation in ground water which may leak into the cavity. For example, tritium will be readily incorporated in water molecules and would therefore be carried with it, as would soluble forms of other radionuclides.
6. Rate of uptake of radionuclides by plants or animals exposed to materials reaching the surface.
7. Biological characteristics of the radioactive elements having their principal effects by incorporation in the body from ingestion of air, water, or food.

8. Adequacy of protection standards for individual radionuclides to which human populations may be exposed. A question related to this last point is the degree of environmental monitoring which will be required to determine the extent of contamination of food, air, and water by radioactive materials released by the explosion.

A detailed discussion of all of these factors would extend this report unnecessarily. I shall illustrate them by considering only a few of the most important radionuclides which may be released from a fission detonation. In the discussion that follows, the assumption will be made that there is no significant release of nuclear debris from the point of detonation except through small openings to the surface through which gases, vapors, or water can penetrate. This assumption obviously is of great importance in defining the health implications of these explosions.

IODINE-131 AND IODINE-133

These two isotopes of iodine are produced with high fission yield, but because of their relatively short physical half-lives (iodine-131 the longest, of 8 days) they will not remain radioactive for longer than a few weeks. Iodine is readily volatilized, and therefore can easily be vented at the time of the explosion. In the atmosphere, it is carried down to the surface of leafy plants, where it may be ingested directly by human beings or ingested by dairy cattle and incorporated into milk.

Wide distribution of this radioisotope can take place from the point of release through wind currents, as, for example, was shown with the windscale release. Iodine is concentrated in the thyroid glands of animals and man. The production of thyroid cancer is the particular health concern, and a short, single exposure may be sufficient. Cases of thyroid cancer are appearing in Marshall Islanders exposed to fallout.

XENON-133 AND KRYPTON-85

Xenon-133 is produced in high fission yield, krypton much lower, but because of the much longer half-life of krypton-85 (10 years versus about 7 days for xenon-133m and xenon-133) the latter is more important for long-term exposures; that is, krypton is. Both of these are gases which can be released at the site either directly through fissures, or through solution in ground water in ultimate release to the atmosphere.

In general the noble gases do not take part in biological reactions and therefore are present in tissues only in direct physical solution. The hazards from them result from external whole body radiation by gamma radiation, and by beta irradiation in the body from xenon. Xenon will be mainly a local problem because of its short half-life, but krypton may be distributed throughout the world's atmosphere in time. The chief health problem from these gases arises because of the whole body dose which will be added to radiation exposures from other sources.

TRITIUM (HYDROGEN-3)

Tritium is produced as a fission product in low yield, and some may be produced also by activation either of natural deuterium or

from neutron reactions with certain mineral constituents of the surrounding rocks. Although produced initially as hydrogen gas, it exchanges rapidly with water to produce tritiated water (a water molecule with one tritium atom replacing a hydrogen). Either as a gas or as water, the tritium could be vented quickly or be incorporated in the ground water and move accordingly.

With its long physical half-life of 12.3 years, tritium can travel over long distances and come in contact with large population groups. Tritiated water is readily incorporated in food and thus in time will come into equilibrium with hydrogen present in both food and water in the diet. Most tritium will be rapidly excreted once exposure is stopped, but a small fraction may be retained for long periods. The principal health problem from tritium in small amounts appears to be from incorporation in the genetic material of the germ cells, particularly the ovaries of females. In this case, tritium laid down in the DNA of ova may remain for the reproductive life span.

STRONTIUM-89 AND STRONTIUM-90

Strontium-89 is produced in large amounts but it has a relatively short half-life of 51 days. Strontium-90, which is produced from krypton-90 decay, has a 28-year half-life and thus constitutes a very long-lasting residue on any nuclear detonation. These two isotopes are not highly volatile and therefore would be expected to remain localized at the detonation site, unless carried away by solution in ground water.

Strontium is generally fairly soluble and therefore may reach the surface in ground water. Once in the environment, strontium isotopes are biologically similar to calcium, which is an essential element for most living forms. Therefore strontium can be concentrated in the tissues of plants and animals in the food chain, although less than is calcium. In higher animals and man, strontium, like calcium, goes preferentially to bone. The principal health problem from strontium ingestion is therefore bone cancer.

CESIUM 137

This isotope is produced with fairly low yield, but, like strontium-90, has a long physical half-life of 27 years. Cesium binds fairly strongly to the surfaces of soil particles, and thus may not easily be transported away from the site of the detonation.

In terms of its biochemical reactions, cesium behaves like potassium, which is an element essential to, and readily taken up by, plants and animals. Thus cesium can easily enter the food chain and will be incorporated in all of the cells of an individual ingesting it. The principal health concern from cesium is on the genetic material of the germ cells. Unlike strontium-90, cesium will be fairly rapidly eliminated from the body if exposure to it is stopped.

PLUTONIUM-239

This activation product of uranium-238 is typical of the heavy elements which may remain after a nuclear detonation. Plutonium-239

has a physical half-life of 24,000 years. It will therefore remain for long periods of time in the vicinity of the detonation, but its physico-chemical properties are such that it is likely to remain insoluble or bound in a form which cannot readily escape into ground water supplies.

In terms of the health implications of plutonium, it is a highly hazardous radioisotope when ingested, the chief long-term effect being bone cancer. If plutonium particles are inhaled and remain for a significant length of time in lung tissues, the likelihood of the production of bronchial cancer is also considerable. (The same may be said for small particles of other radioelements, particularly beta emitters such as strontium–90.)

There are a number of activation products which may be produced by an explosion in significant amounts. Included in this group are calcium–45, iron–55, and phosphorus–32, all of which are very important biologically. The quantities produced will depend on local conditions.

FUSION DETONATIONS

In evaluating the public health aspects of a fusion or thermonuclear explosion, the same considerations apply as described above for fission reactors, except that the situation is somewhat simpler because the products of the thermonuclear reaction are in general much less complex than for fission.

A small fission explosion is required to initiate the thermonuclear reaction, and for this portion of the detonation the same factors discussed above apply. With this exception, the only radioactive products of the fusion reaction are tritium and radionuclides induced by the neutrons produced. The amounts of tritium produced are enormous—in curie quantities, about 10 times as much per megaton TNT equivalent as all of the longer-lived fission products from a fission reaction combined.

The comments that have been made above with regard to tritium release, particularly in ground water where it may come in contact with human populations, applies in this case also. It should be noted that of the long-lived radionuclide products of a nuclear detonation, tritium has the highest probability of escaping from an underground site.

The biological effects described above vary considerably from one radionuclide to another, and the question arises concerning the extent to which the effects of one will add to those of another or be independent of them. There will be some additive effects particularly from the gamma-emitting radionuclides such as iodine–131, krypton–85, xenon–133m, and cesium–137. All of these will give a radiation dose to the whole body if the individual is exposed to them in air, water, or food.

Similarly, tritium, although it emits a very low energy beta particle, may produce a whole-body radiation effect because of its distribution in body water. In addition, the strontium isotopes, although localized strongly in bone, nevertheless may be taken up in other tissues to a lesser extent.

With respect to cancers in a specific tissue, however, there is less likely to be an additive effect of the different isotopes unless they

localize in that tissue. For example, the production of thyroid or bone cancers from iodine or strontium exposure is unlikely to be greatly affected by the presence in the body of genetically significant amounts of cesium–137 or tritium. This matter is important in considering the health implications of exposure to several of these radionuclides together rather than individually.

The standards adopted by Federal and international agencies concerned with radiation protection to the population at large are based on the idea of a maximum permissible dose to the body tissues. Generally this permissible dose is expressed in terms of a dose rate, such as in millirem per year. (The unit of millirem incorporates the average physical dose corrected for biologially modifying conditions or circumstances, such as the way in which radiation gives up its energy in the tissues.)

The concept of an average physical dose to a tissue or to an organ is not necessarily meaningful when applied to effects on individual macromolecules in cells or when the radiation source is sharply localized, as for a radioactive particle of about 1 micron in diameter.

The question of the effective radiation exposure to a macromolecule is important when the genetic changes in the DNA of the germ cells are considered. The genetic hazard is the limiting consideration for the exposure due to their employment in nuclear industries—and the aim of the standards is to restrict exposure of people in the population at large to less than a total cumulative radiation dose of about five rem (5,000 millirem) during the 30 years of the reproductive lifespan.

It is of interest to note that in 1959 the International Committee on Radiation Protection recognized the probability of radiation exposure from sources other than environmental radionuclides and apportioned the permissible gonadal radiation exposures to several possible sources.

This principle has not extensively been applied either to the possibility of exposure to several radionuclides nor to the increasing use of nuclear fuels for power. In addition, any additive effects of other mutagenic agents which will also be increasing in the environment must be taken into account.

The evidence to date is clearly inconclusive in regard to the possible genetic risks from exposures to man-made radiation, but the indications are that a total gonadal dose during the reproductive lifespan of about 5 to 10 rem from all sources will increase the mutation rate by about 10 to 20 percent. Such an increase will not result in an equivalent increase in heritable mutations, because, as for spontaneous mutations, many are lethal.

Whether or not this is an acceptable risk is not a scientific decision but instead is a philosophical or a political one. In the ICRP report mentioned above, the permissible gonadal dose suggested for internal emitting isotopes was 50 millirem per year for the general population, or 1.5 rem in 30 years. External radiation exposures from radioisotopes were assigned one-third this amount. I believe that this more conservative view, which has not been pressed further by the ICRP since 1959, is preferable to assigning the full permissible dose of 170 millirem per year to each source of radiation, and requiring that the permissible concentrations be lowered when other exposures to radiation are present.

In addition, I think we must reach some agreement on the possible additive effects of exposure to several radionuclides simultaneously. In this last instance, I believe we can be more lenient than present Federal regulations permit.

TRITIUM STANDARDS

With respect to tritium standards, I believe they are too high and tritium is more hazardous than is presently thought to be the case. This particular issue is important in this context because of the choice available in using thermonuclear or fission explosions for these purposes. Obviously the possible health risks to be balanced against the benefits of using nuclear detonations will depend largely on the hazard from tritium for thermonuclear detonations.

PUBLIC HEALTH IMPLICATIONS

The public health implications of underground use of nuclear detonations for engineering purposes leads to some important considerations:

1. Monitoring of the environment likely to receive significant amounts of radioactivity from the explosion will be required for long periods of time.

2. In terms of radiation exposure to large groups of human beings, this application of nuclear energy may be, in a sense, competitive with exposures to wastes from nuclear reactors.

3. Because underground uses of nuclear detonations may result in radioactive materials being transferred across national boundaries, either in air or water, there are obvious international implications to this development.

4. In evaluating the proposed benefits from the use of underground explosions, a better mechanism of providing for participation of public representatives would help to assure that these benefits will be fully understood by the populations potentially exposed to the risks.

This last point is directly related to the recommendations contained in S. 3042. I support the intent of this bill, and I do so as a supporter of the Atomic Energy Commission, not as an opponent. Retention of the complex issues of environmental health raised by these uses of nuclear explosions in one agency of the Government is to invite a continuing and unproductive argument against that agency. Indeed, in my opinion, unless State and local representatives become more closely involved with the issues, a growing "credibility gap" between the Federal agencies and the people will be increasingly likely.

Thank you, Senator.

Senator MUSKIE. Thank you very much, Dr. Radford. With respect to the last point you made, which is clearly an emphasis upon the desirability of public understanding of the risks associated with these explosions, I gather, from what you said, that there are risks involved even within the limits, even if the explosions operate within the guidelines set by the Radiation Council.

Dr. RADFORD. That is correct, Senator. This is based on the idea that there is no threshold for radiation effects. Now, I understand that this question of the threshold is again being brought up by the

Atomic Energy Commission, Division of Biology and Medicine, for certain types of genetic effects, but I subscribe personally to the notion that there is no threshold and therefore any increase in radioactivity carries a small risk.

Senator MUSKIE. So that any discharge, however small, carries some risk for some human beings?

Dr. RADFORD. That is correct.

Senator MUSKIE. And what you are saying is, that before anyone should be asked to accept these risks, we ought to understand what they are and anticipate in some fashion in public hearings, as you suggest, any policy decisions that generate that risk.

Dr. RADFORD. Yes. Obviously as a practical matter, this has to be done through the public representatives.

Senator MUSKIE. Did you have an opportunity to study yesterday's testimony before this committee? It didn't receive much coverage in the press, so I don't know if you had this opportunity. Are you familiar with the testimony given by Dr. Gofman and Dr. Tamplin?

Dr. RADFORD. No, sir, I am not.

Senator MUSKIE. Let me read you the opening sentences.

We wish to apprise you that in our opinion the most crucial pressing problem facing everyone concerned with any and all burgeoning atomic energy activities is to secure the earliest possible revision downward by at least a factor of tenfold of the allowable radiation dosage to the population from peaceful atomic energy activities. The Federal Radiation Council allowable dose of whole-body ionizing radiation is 0.17 Rads per year. We shall present to you hard evidence that leads us to recommend that this be reduced now to 0.017 Rads or even less. And we shall present to you the estimated disastrous consequences to the health of the public if this recommendation receives less than immediate serious attention.

If you have not had an opportunity to consider that proposition before, you may not want to react now, but I wonder if you might.

Dr. RADFORD. Well, of course, I have considered this at some length, because it is implied to some extent in my discussion of the radiation standards. First with regard to the fact, the statement isn't quite true. The whole-body permissible dose is actually 0.5 rads per year. The gonadal dose or the genetically important dose is 0.17, and I presume that Dr. Gofman and Dr. Tamplin were discussing the genetic implications primarily, so that the numbers they have expressed would apply.

This whole issue of what is the acceptable level that we should permit is, of course, central to many of the arguments that are going on with regard to nuclear reactors as well as this use of nuclear fuels. I think ultimately that those of us who are scientifically and technically concerned with these issues have to make our individual judgment, and, in part, this was underlying why I said that the acceptable risk is really not a scientific decision at all; it is a philosophical or political one in a sense.

There are some practical limitations. The Atomic Energy Commission, for example, uses as one basis for defending their position—or perhaps I shouldn't say "the Atomic Energy Commission"; I guess it is the Federal Radiation Council—the view that even if everyone were exposed to this increase in radiation to the gonads, it would not be possible to detect a significant effect of it on the human population that is exposed.

In other words, if it is not measureable, then this carries the implication that it is not as hazardous as other people might think.

Now, there is some validity in this point of view. For example, in the whole field of environmental health, and I am now talking about exposures to all the other things that we may have descending on us: microwaves and various chemicals which are being produced in great array and bewildering complication; as we look at these issues, we have to ask ourselves the questions: Are the people suffering? Do we detect any effect on the population as a result of the presence of these that we can identify?

This is, for example, a very pertinent issue today in the air pollution field, the question being: How much damage is the presence of different kinds of air pollutants doing to the human population? And if it is difficult to show that there is a health effect, we tend to take the view that it perhaps is not so serious as we might otherwise think.

I would contrast this situation to the tremendous increase in lung cancer that has occurred, some of which may have been due to air pollution. In this case, we can point to a very definite change in the health status of the American public and, in fact, the world's population that has occurred in the last 30 or 40 years.

So, obviously we have a kind of alarm bell when we see something like that which indicates something has changed and something is wrong and we had better do something about it.

On the other hand, if we can't see any effect, and as far as we can tell the population is the same today as it was 30 years ago, or would be 30 years from now even if radiation exposures did go up, then we might be inclined to be rather more comfortable about it.

However, looking at this issue from the other side of the argument, the difficulty is that in the first place our knowledge of human genetics is really very rudimentary at this stage. We don't have a clear idea of what the normal mutation load that is carried by the human population is, not even by a factor of 10 perhaps.

So that the fact that we can't show any change merely may mean that we don't really know very much about it now, and don't have the tools to study it adequately.

This argument, in other words, comes down to the fact that we do have to be very much more cautious in trying to restrict genetic exposures to any mutagenic agent, whatever it might be, whether it is radiation or certain types of chemicals that we know will produce mutations.

Thus, I would say that caution is warranted, and I think that those who are concerned with the regulations also feel they are taking a cautious stance.

Now, the basis of the experimental evidence on which this number of 0.17 per year was developed in primarily the work of Dr. Russell on mice at Oak Ridge. This is an enormous, painstaking job done by Dr. Russell, and he has the admiration of the scientific community for his efforts.

On the other hand, it raises a strong issue of how good a model for the human population is a mouse, just as the same issue was raised about how good a model is a fruit fly for the human population. We recognize in biology that one mammal is a better model for another mammal than is a fly or insect.

But still there are some disturbing questions here, and I have discussed these questions with some human geneticists at some length. The first question has to do with the reproductive span. We have reproductive life span up to as much as 50 years from the time of first laying down of the germ cells.

The second difference is that in human populations a very strong effect of age of the individuals can be shown in human mutations. That is, certain types of genetic mutations are much more frequent in older parents than they are in younger parents, and this effect has been interpreted as an indication of a kind of random loss of integrity of the genetic material with the passage of time.

There has been not well documented any significant age dependent effect in mice or in other short-lived animals, so that there are reasons to suspect that if you irradiate, say, human ova—the effects may be different. I have been particularly concerned with this because the ova that are deposited in the ovaries in the child in-utero remain quiescent. They are the stock of the 400 or so eggs that will be discharged by the female during her reproductive life span, and they remain in a quiescent state; they are not dividing, are not diluting out any contaminants coming in, and if radioactive materials are laid down in certain molecules they presumably may stay for the whole life span.

Now, the point is, if you have a small amount of insult to the genetic material of the eggs over a period of time, it is at least conceivable that it might be synergistic with the so-called random thermal motion effect, whatever that is in man, which would not be as important in an organism like the mouse.

So these are some of the reasons why I feel that, if anything, we do have to be somewhat more conservative, and, of course, the Federal Radiation Council's guide does take this into account. According to Russell's calculations, they have already a safety factor of something like 10 built into it, meaning that slow radiation exposure is perhaps one-tenth as hazardous as would be a single acute dose of radiation to the gonads.

That is a long answer to a short question, and I am not sure I have answered it completely. Perhaps if there are other questions? I suppose you want me to come down to a number.

Senator MUSKIE. Well, if you think you can. May I say in addition your answer may be more complete than I understand it to be. You are talking to a layman. In any case, to the extent that you want to make your answer more precise, it would be helpful, but I am not trying to force an answer that you can't support.

Dr. RADFORD. In going over this issue in the last year, I have recognized that obviously, to comment on these things publicly, one must settle in his own mind the best possible answer to the issues.

Senator MUSKIE. May I say this before you continue: that my impression from yesterday's testimony is that there is at least one reaction from the Radiation Council to take sharp issue with the testimony yesterday, so I suspect that that issue will be drawn at some point in these hearings, and if you wanted to add your point of view, it would be very helpful.

Dr. RADFORD. I have already indicated in my testimony that I look upon the 1959 recommendation of 50 millirems per year gonadal dose,

from internal emitters—or 70 from both internal and external sources from environmental radionuclides, as a reasonable basis for taking into account the possibility that the individual may be exposed to radiation from other sources; not only from medical X-rays alone, although that is another source, too, but also from television sets and inadvertent exposure from radiation.

So this is an apportioning out of the value of 170 millirems to a variety of sources.

Now, within that figure of 50 millirems per year, we have to recognize that we may have several sources of internal emitters, and this use of nuclear detonations brings that point out, I think, rather clearly. Therefore, the question is: How much do you assign any one of the possible sources of internal radiation?

Here I tend to be perhaps somewhat more conservative, as I indicated in my testimony. I do not think we have to say that you add them all up, and if you were exposed to equivalent rad doses from all the different internal emitters, that you would then divide by that number, whatever it might be—say 10—which would then allow only 5 millirems per year from each one. I think that is going a little too far.

My own feeling is that I would divide it by 3. Now, it just happens that when I do that, I come out with 0.017, but I have reached that point by a somewhat different route, I think, than Dr. Gofman and Dr. Tamplin did.

But I would have to be honest with you and say that that is a kind of philosophical decision. I can use figures to estimate what the mutation rate would be, as I have done here, and say that 5 or 2 percent or 1 percent increase in mutation rate is acceptable to me, at least as a member of the public at large that has to accept this risk, and it certainly sidesteps the issue of whether one could ever detect an effect, because, as I indicated, I don't think that is a completely compelling argument.

ACCEPTABLE RISK STANDARD

Senator MUSKIE. To try to put in layman's language what you have said this morning, it seems that the limits set by present guidelines do not eliminate risk to human beings, so that the standards do not assure safety. The setting of standards is not a scientific decision, the product of scientific application, but instead a philosophical or political decision by somebody that the standards represent an acceptable risk. From the scientific point of view, there is risk involved in the present standards for human beings. Now, is that an accurate layman's summation of what you said?

Dr. RADFORD. Yes; that is very good, Senator. Let me point out, though, in defense of the individuals who are put in the "hot seat" to make these kinds of judgments, that we are going to have to accept a risk. We accept a risk from many other kinds of things. We may have to accept a risk in the alternative. Let us say we didn't use nuclear detonations for these purposes but used some other means of achieving the same end for the benefits of the public. The construction industry, as you know, has not a very impressive safety record, so that a fair number of people are killed or seriously injured on construction projects. These might be avoided by this application.

Senator MUSKIE. The difference is that other forms don't include genetic risk.

Dr. RADFORD. I agree in this regard that this is a new feature in the picture. Genetic implications are going to turn out, I think, to be a very difficult political, social, and psychological issue with which to grapple in terms of setting standards. I want to emphasize, however, that not only radiation but other possible agents will have this feature.

We have already seen the big furor that has come up with regard to cyclamate removal. Now, this was not strictly a genetic risk in the same sense we are talking about here, in terms of transmission to the next generation, although it is conceivable that cyclamates could have this effect. What had been demonstrated was a teratogenic effect that has produced abnormal offspring because of abnormal development in the fetal stage.

But this is the kind of very difficult question that we are starting to face, and I think it is applicable to a great many of these environmental problems. I would say with regard to the risk question, your summary is correct. The question of what is an acceptable risk, as I have already indicated, really is a political issue. It is based on a scientific presentation; that is, the scientists do their best to tell you that there will be a probability of a mutation rate of a certain amount, a certain percentage of the live births per year will have mutations, and a certain percentage of births that would have been live won't happen, because there was a lethal mutation.

All of these things can be stated in scientific terms. Then it is up to the public representatives to make a judgment as to whether this is acceptable or not.

Senator MUSKIE. Of course, we all understand the risks involved by reason of being alive. The automobile, I guess, perhaps is the most dramatic example of a risk—50,000 deaths a year—but we understand that figure; we understand all of its implications, and we have some feeling, I guess, as owners and operators of motor vehicles, how much risk there is involved for us individually, and I suppose there is some feeling that "It can't happen to me."

So those kinds of risks are involved, and, of course, these kinds of risks are in a different category. First of all, of course, the fact that there is genetic risk involved which the layman finds it difficult to understand and comprehend. Secondly, because a scientific presentation is involved. By the time you fellows get through talking, you know, from the point of view of most lay citizens, all they are looking for then is your conclusion.

So in order to satisfy a layman or citizen, you have to do more than make a scientific presentation; you have to make the philosophical or political judgment which translates the scientific presentation for the layman. It is in that area that people get perhaps unnecessarily alarmed of the implications of nuclear explosion.

Yesterday, our two witnesses, Dr. Tamplin and Dr. Gofman, questioned the wisdom of any technology which would allow uncontrolled release of radioactivity in the environment. I would like to get your reaction to that from a scientific standpoint.

In other words, since we are dealing with quite precise standards of measurement, even though they still involve risk, isn't there a threat

in terms of public safety if we are dealing with uncontrolled releases of radioactivity? In other words, if the safety factors involved are related to such precise standards—the .017 rads; that is a pretty small figure—then don't we need to be concerned about any release of radioactivity which is uncontrolled?

Dr. RADFORD. Yes, sir, I agree, and this is one of the things that worries me with regard to this particular application.

Let me draw a parallel. When this kind of argument is presented with regard to the nuclear reactor development, the Atomic Energy Commission specifically points to past history, or the record. This is a perfectly valid approach. They say there have not been any significant releases from excursions in commercial power reactors and we have a number of reactor years of experience already accumulated. I think this is a valid defense.

On the other hand, what is the record with regard to underground detonations? Here I don't think it is quite so comfortable. Use of the Nevada test site for underground detonations, had to do with the partial abrogation of the nuclear test moratorium, but also it was recognition that those testing weapons had been criticized about worldwide fallout from the surface testing of bombs and they went underground.

Even so, there have been releases from these detonations—and I am sure you could obtain better witnesses than I with regard to the specifics of these. For example, Salt Lake City, Utah, had a significant rise of iodine-131 from underground tests—I can't recall the exact year: I think it was 1962—where it had reached the stage that they were considering preventive action which would have meant perhaps elminating milk sources, any vegetables that had been contaminated, and so on. It had reached a point where the health officials had been alarmed.

Now, to my view—which is essentially a lay view in this situation; I have no specific knowledge—that is an uncontrolled release. They thought they could contain it underground, and it didn't stay that way. I wonder therefore, if we are really going to use nuclear explosions for engineering purposes, how many real applications you have where you can explode them so far underground that one can guarantee complete containment.

The other question is, as I have tried to stress in my testimony, anything that can get into the ground water supplies is not going to be contained. Eventually it will leak out. If it has a long enough half-life, it is going to be distributed into the surface water. So I do think there is a very definite point here about how readily these nuclides can be contained within the actual detonation site.

Senator MUSKIE. In other words, underground detonations must be concerned with two control points: one, the limits of acceptable risk, limits to which human beings ought to be exposed; secondly, the inputs into the environment; and if there is to be minimal testing, on both points, there must be control.

I notice you were very careful at the outset of your statement to make this point when you said: "In the discussion that follows, the assumption will be made that there is no significant release of nuclear debris from the point of detonation except through small openings

to the surface through which gases or vapors can penetrate." Even in that type of explosion I suppose there is not total control.

Dr. RADFORD. That is right.

EXCAVATION

Senator MUSKIE. I suppose the next obvious question is whether or not you think it is possible to proceed with the Plowshare plans and especially with the excavation-type explosions with safety.

Dr. RADFORD. Well, I would question the kinds of excavation. Now, the sorts of things that were done with "Ruleison" or "Gasbuggy" were intended to improve the recoverability of natural gas. That is one particular kind of benefit. I am not sure how much benefit, but that can be argued by others.

But if you are talking about excavation, I can't help but feel that you are talking about reaching to the surface somewhere. If you are going to use a bomb to do this, ultimately you are going to break through into an excavation site. Otherwise, why would you do it? So from that perhaps naive point of view, I would say you have then long-lived radionuclides that are going to get distributed around.

Senator MUSKIE. I am as naive as you are on this point. Seemingly in such proposals as the excavation of the Panama Canal, we are going to open up 40-odd miles of ditch that has been exposed to nuclear explosions before being opened up, and which will result in releasing radioactivity into the atmosphere. I am glad to note that you have the same questions at least. Have you had much opportunity really to study the aftereffects of these various underground nuclear explosions?

Dr. RADFORD. The only report that I could put my hand on very easily was the recent study of Project Gasbuggy in the Public Health Service Reports for July 1969, actually pointed out by Mr. Grundy. I have looked at this and I am surprised at the lack of information that came out in that particular report about uptake in the ground water. This report was related to the gaseous composition, in other words, the composition of the gases in the cavity, so I am not sure of the extent to which environmental monitoring has been done on that one.

Senator MUSKIE. Senator Gravel, do you have any questions you want to put?

Senator GRAVEL. Yes, Mr. Chairman.

Doctor, you testified admirably in favor of our proposed commission. The commission may be an extension of what we are doing right now, which may not add to our total accumulation of knowledge.

I wonder if, due to the fact that we have drawn the same conclusion that there is not enough information, if a study, a further study on the radiation effects might not be called for at this particular time. What would be your comment on that?

Dr. RADFORD. Do I understand, Senator Gravel, that your question is related to the health impact of these explosions or to the engineering considerations?

Senator GRAVEL. No; the health impact, the radiation dangers involved in uncontrolled radiation related to Plowshare excavation programs and the radiation from atomic reactors, although we have a

greater amount of experience in that area. I am concerned with the statement you made earlier that we are basing our levels of radiation upon two particular important studies in the field, but you raised the question: Are these studies sufficient?

Dr. RADFORD. Well, I would say that there is definitely more work needed. I personally have expressed the opinion to representatives of the Atomic Energy Commission that a study of the genetic implications of random labeling of gonadal hydrogens by tritium—that is a fancy way of just saying we get tritium into these cells in the best possible fashion you can—so that all parts of the cell are equally probably labeled, that long-term studies of effects of this kind of radiation exposure will be needed.

There are technical difficulties. Dr. Totter in particular is not very enthusiastic about this, and if I were in his shoes, I might not be, either, because it is not going to be an easy kind of experiment to do.

But in my opinion, because so much of the whole nuclear program hinges on the relative hazard from tritium exposure, and if we go to fusion reactors we will be generating much more tritium, that I think this is worthwhile.

So that we have, in other words, additional work to do. Now, I shouldn't leave the impression that the only work that has been done on genetic effects that is pertinent is Dr. Russell's work. There are many other investigators who have carried out experiments similar to his or related to it. So we have a large body of information.

But I think we are going to need much more. I think we need much more knowledge with respect to human genetics so that we can understand better when people do carry mutations that are possibly deleterious.

All of these studies in general are expensive, but in this case unfortunately we are in a period of cutting back on biological and medical research, so that rather than going in the direction of increasing effort, we are perhaps stabilizing or going down.

The other aspects of the health problem has to do with the way these isotopes move around once they are released. That is the environmental surveillance issue, and the particular July report that I referred to is one means of publication of the surveillance work that the U.S. Public Health Service is doing.

But if you look critically at the data, not only on the results of "Gasbuggy" but a lot of other environmental surveillance work, there, are gaps. We would like to know better what kinds of food sources, for example, become contaminated with different kinds of radioactivity. A lot is being done, but in trying to get answers that will satisfy the public and satisfy the scientific community, too, we need more answers.

INVENTORY OF CONTAMINATION

Senator GRAVEL. Is it possible in this surveillance you talk of to develop a total inventory of the contamination of our environment with respect to radioactivity or, if not monitor, let us say, secure information not only from our activities but activities of other nations? Do you feel this is possible, to monitor or develop inventories to keep a running accumulation as to what we are doing? Is this a scientific possibility?

Dr. RADFORD. I rather doubt that it is a scientific possibility.

Senator GRAVEL. We keep talking about throwing radioactive pollutants into the environment and we dispel our fears with the simple statement that it is being diluted. I am no scientist, but I know if I take a solution and keep dumping something into it, some day that solution is going to be at the level of what I am dumping into it, and in simple terms, isn't our total environment that way? If we keep throwing in this radiation and it has a life expectancy, then don't we really alter our environment? I am concerned over the increasing rate now and that rate projected in the future.

Dr. RADFORD. Your first question was: Could we develop a complete inventory? And I said I didn't think it was scientifically possible. On the other hand, in the way you just phrased the question, I would say that by developing a better understanding or, as we call it in science, a better model of the way these things move, we can station ourselves at certain critical points and be able, by detecting what is present, say, in various kinds of food sources—animals or plants—and by measuring these accurately, we can get a good network which will enable us to be able to say that the probability that anything serious is occurring is .01, or something like that. That is perhaps the best we can do, but that is an acceptable risk, too, another variety of it.

In other words, by sampling enough different points, we can get an idea that the Alaskan crabs, for instance, are not becoming so radioactive that they are going to be banned from sale. Do you follow my intent, sir?

Senator GRAVEL. Yes, I do. This leads me to the statement that you made earlier which I think is frightening; that is, if we are decreasing our effort in the study in search of the knowledge of the effect and then increasing the cause that produces the effect at a faster rate because we need the power. I just make that as a statement, not as a question.

But let me ask this: I think the public, by and large, is relying upon the Radiation Council to a greater degree than is realized, because when you do make the argument that you make in your last paragraph—which is the argument that I make, and I want to associate myself very strongly with that because I am not anti-AEC as people have felt in the past. I had a measure of concern that people who prosecute this program and at the same time are responsible for the safety, the total effect of this form of energy, have a dichotomous interest which is not reconcilable. I see you share the same view.

But the argument is, of course, that everything is all right because they are adhering to these radiation standards. Two scientists yesterday took exception to the existing standards.

Since you have taken that strong an exception to these standards, how do you view the creation of a mechanism, what kind of mechanism would it be, where would we house the responsibility for policing the environment if not through the setting of radiation standards, and what could we do?

Dr. RADFORD. Well, I think we will have to set radiation standards, and these standards will not be zero, or I don't believe they should be. In other words, I am responding to Senator Muskie's earlier comment, that I don't think we can say "no risk at all," which would, in effect, just ban the use of nuclear energy, period.

I think we have too many benefits to gain from the use of nuclear power certainly, that we are going to have to go ahead on the basis that we accept the risk, so there will be standards.

The development of the Federal Radiation Council, of course, was an attempt to take it out of the realm of any one particular agency and, of course, it did have this effect. However, the Federal Radiation Council, as presently constituted, does not have a large scientific staff, and they, in effect, adopted the recommendations of the National Committee on Radiation Protection and the International Committee on Radiation Protection.

REVIEW OF STANDARDS

Senator GRAVEL. Just one question here. When is the last time these standards were reviewed, reset or given some real in-depth consideration? Because if the council is using the research of others and the council has no money to look into this properly, then what about the people that they took the information from?

Do they have any money or is this something somebody dreamed up 10 years ago that has been accepted?

Dr. RADFORD. Well, no. The International Committee on Radiation Protection has fairly frequent meetings. I am not quite so familiar with the operation of the national committee. I assume they do, too. I have the very definite feeling that once the fallout issue subsided that the question of reviewing the standards for general population protection went into some degree of, I wouldn't want to say inactivity, but a lesser activity. I think the feeling was that the standards that had been established were sufficiently well documented. In a sense I am answering your question by saying that probably in the last 10 years there has not been a great deal of review, but I do know that the ICRP has been sending out reports regularly related to many issues of this kind and have considered this matter of the acceptable genetic dose.

But I think that the problem here is that the opportunity has been limited for expression of different points of view. Let us take, for example, Dr. Tamplin and Dr. Gofman. What would be the mechanism for them to present their point of view to a body at this time? I am not familiar with it.

It may exist, but it would not be, I think, particularly, profitable for them to go to the Federal Radiation Council because these are people in the executive branch primarily, who are not scientists.

So I think that in answer to the long question; namely, How do we develop the mechanism?" there ought to be opportunities to bring up questions on these standards. Now this is done for chemicals.

The Committee of the American Council of Government Industrial Hygienists publishes regularly lists of maximum permissible concentrations of a wide variety of chemicals. When there is any question of reconsidering these, there are scientifically open meetings for any people who have any interest in whether the level should be lower, higher, or in between.

There is an opportunity to come together and discuss it scientifically. In many cases representatives of industry that may feel that the standards are too stringent will have an opportunity to say something

about the standard. In the case of air pollution standards that are now being set up all over the country, there are public hearings where the standards are presented and the public at large has an opportunity to come and say whether they think they are good or bad.

Usually, of course, it is a scientific evaluation and they also invite scientific evaluation from a number of people. In the case of the radiation field, this grew up as a very special group, definitely disassociated from the medical profession because except for radiologists the medical profession never really understood very much about ionizing radiation.

We have then a group which has done a marvelous job, the standard setting groups. I take my hat off to them. They had a formidable job to do and they have done an excellent job in setting these standards. But as I have already indicated I think tritium is one isotope that has not been given a close enough look and I am recommending that they do so.

I understand there is a committee being assembled to do just this, but I am not on the committee and I don't know that I will have a chance to present my views except by publishing a scientific paper, which I intend to do.

Senator GRAVEL. Mr. Chairman, I would hope at some point we could hear from somebody from the Federal Radiation Council.

Senator MUSKIE. That is an approach.

Senator GRAVEL. Thank you.

ACCEPTABLE RISKS

Senator MUSKIE. I think you made two points, Doctor, with respect to acceptability of risks. In the first place it seems to me there is some evidence that we can become insensitive to risks, which initially alarmed us, learn to live with them and as a result, perhaps, tend to accept risks evidently that we ought not to.

I think that is a relevant point to make. Second, in our eagerness to press forward with new technology, it seems to me there is evidence that we are willing to accept a higher level of risk at the outset in order to press forward with technological advance before we have the kind of complete information we ought to have.

I think these two tendencies were at work in relation to pesticides. I think they are working now in the field of thermopollution from nuclear powerplants.

I think they tend to work every time we see the opportunity to take advantage of some new development chemically or technologically in the name of progress that these things happen to us.

As a result we see a constant deterioration of the quality of the environment and its lifegiving potential in the name of progress. This is what concerns us and what I think has triggered escalating public concern with air pollution, water pollution, and other environmental insults.

Let me ask you your evaluation of the significance of the developments in this connection: First of all the fact that the limits of acceptable risk from radioactive nuclear explosions have been constantly dropped which, it seems to me, illustrates one of my two points.

Second, the testimony we received yesterday that the medical profession has undertaken to introduce practices which will limit the exposure of human beings to radioactivity from X-rays. Both of these developments, it seems to me, emphasizes at least one of the two points I made which results from the acceptance of too great risks initially in order to take advantage of technological development.

So I wonder if you would evaluate those two points I made with respect to the decline in acceptable limits and, two, the concern of the medical profession with technology that has been with us a long time, the X-ray?

Dr. RADFORD. I would be glad to do so. This is a very important point. First, with regard to the lowering of the standards of exposure, the initial concern in setting these standards was with people who were occupationally exposed. That is, they were primarily applied to individuals who were either working directly with X-ray machines, for example, or who were in the military assisting in the bomb tests, and so forth.

These kinds of concerns were with adults, people who could be told that there was some risk here and who could then say, all right I will accept it. They may accept it because of their employment.

So the initial standards dealt with occupational exposures. When it became apparent that the development of nuclear energy was going to involve large human populations that had no occupational concern, then this put a different complexion on the situation, and this was one of the principal reasons that the standards were lowered.

The slow increasing awareness of the genetic implications is another reason. Even so, the defense can be made by the Atomic Energy Commission that there has not been proved to be any genetic or other health implications from environmental exposures, other than from such things as the Hiroshima and Nagasaki bombs.

We also have the question of whether one should restrict the development of a technology because of concerns that may turn out to be either illusions or possibly valid.

Senator MUSKIE. Doesn't that depend in part upon whether or not you are risking something irreversible?

Dr. RADFORD. I think so; yes. I think this is a major point that distinguishes some types of risks from radiation from others. Let me point out that with regard to the medical profession, it is very interesting to see the impact that the real concern for genetic implications of X-ray radiation from diagnostic and therapeutic purposes has had on the medical use of X-rays.

Unfortunately, this concern has had not enough effect on the medical profession as yet. There are still a great many diagnostic units that are inadequately shielded and are delivering a much higher dose than is necessary. The radiation exposure to the population of the United States is higher from medical X-rays than it should be, so that some parts of the medical profession have become aware of the problem, others have not.

The fact is that the conditions for which one may expose people to X-rays have definitely been diminished. For example, sending around mobile X-ray units for tuberculosis detection is now considered not good practice unless there is a specific population that you know has a very high probability of tuberculosis. Then you may undertake it.

But mass surveys of the type that were being done 10 years ago are not considered sufficiently worthwhile to offset the risk that is present. Let me just comment a little more broadly on this issue, Senator, of at what point do we accept the risks from a technology, and your earliest comment that we tend to become rather numb to the risks after they have been around for awhile.

I think this is a very human response and I am afraid we all do go through this process. For example, the risks from carbon monoxide in our environment are, I think, probably more serious right now than the potential risk from ionizine radiation in the environment, if you want to put it on some kind of numbers of people involved or the potential significance of it.

Yet, we are probably a long way from banning the use of automobiles in downtown areas, for example, which would have significant impact on this particular environmental problem. Why? Because we are used to what is there. This fact gives rise to the feeling on the part of the nuclear proponents that they are being discriminated against, because they are new in the game and the other old polluters that we have been living with so long are not being given equal attention.

Senator MUSKIE. Perhaps the point to be made is that before we adopted the internal combustion engine we should have done the sort of soul searching we are now doing with respect to radiation.

Dr. RADFORD. Yes, and I heard recently of a man who commented in 1910 that one of the things that was going to happen as a result of the development of the automobile was that there was going to be a lot of stuff put in the air. His was a voice crying in the wilderness then.

We are, I hope, developing to the point where we can begin to pay attention to all of these matters. It is no defense to say that the amount of sewage that is being put into the Chesapeake Bay from the city of Baltimore is a far worse environmental health problem than the amount of radioactivity that might come from the Calvert Cliffs plant.

This may be so, but it is no comfort.

PROJECT GASBUGGY

Senator MUSKIE. We touched upon two areas of uncertainty that alarms the public and I suspect alarms the public increasingly. One is the effect of radiation on public health generally, and particularly the genetic effects; and, two, the uncertainties as to the controlability of the nuclear explosions.

Now, there is another area of uncertainty that I would like to touch on briefly—uncertainty as to whether you are being told the truth about the danger. You referred earlier to the project "Gasbuggy." We have one report on this project in the Public Health Service's publication called *Radiological Health Data and Reports.*

We have an advance copy of an article that will appear, I gather, in the December issue of this PHS report. The project Gasbuggy was an experimental shot to evaluate the radiological dangers that arise from the use of nuclear explosions as a problem for gas pools.

The opening paragraph of the report touches upon two areas of risk; one, whether or not there was radioactivity introduced into the environment as a result of project Gasbuggy, and the finding was negative.

The other was whether or not natural gas-producing wells near the Gasbuggy experiment were contaminated with radioactivity, and again

the finding was negative. But there is nothing in this report to tell us what the implications were for the gas pool that was tapped by the Gasbuggy project.

In other words, the gas produced by this explosion developed radio-activity and became a concern from the public health point of view. I haven't found anything in this report that touches upon that question. I wonder if you found anything on this question in the report (if you have seen it), whether in your judgment there would be any radioactivity introduced into the well itself by the nuclear explosion, and what you think the public health implications are from the use of gas tapped by such explosions.

I think this is an important question. The AEC tells us of 100 appli-cations for the development of gas fields that are possible by use of this technique, and so we ought to know whether or not the gas pro-duced by this technique is usable, since it may involve risk to the public health.

Are you in a position to enlighten us on that?

Dr. RADFORD. Well, I can only answer in relation to a rather hasty survey of that July 1969 report, which was addressed in part to the question of whether the various hydrocarbon fractions that were detectable in the gas fields had any radioactivity in them. They found, as one might expect, that tritium did exchange with the hydrogen of methane, and a number of the other hydrocarbons. I can't cite the whole list.

(The following report was later supplied for the record:)

STUDIES OF CHEMICAL AND RADIOCHEMICAL COMPOSITION OF NATURAL GAS FROM THE CAVITY PRODUCED BY THE PROJECT "GASBUGGY" NUCLEAR SHOT[1]

(*Charles F. Smith and Floyd F. Momyer*[2])

Data relating to the chemical and radiochemical results from Project Gasbuggy
chimney-gas samples obtained prior to 210 days after detonation (December 10,
1967) are presented for significant non-radioactive components of the gas and
for tritium and krypton 85. A discussion of changes in composition occurring
during the flaring of 5×10^7 ft³ gas in the late spring of 1968 is included. Some
interpretation of the observed changes is advanced but with the data now avail-
able, no definitive conclusions seem warranted. This is a status report of the con-
tinuing effort to define and understand the chemical and radiochemical aspects of
project Gasbuggy.

Major constituents of the Gasbuggy gas during the period from 34 days to 200
days following the detonation were methane (increasing from 37 to 44 percent),
ethane (increasing from 4 to 5 percent), propane (constant at ~1 percent),
carbon dioxide (constant at ~36 percent), hydrogen (decreasing from 17 to 12
percent) and carbon monoxide (decreasing from 4 to less than 2 percent). Much
more significant changes were observed during the first month. The major
reactions used to explain these trends are:

$$CO+H_2O \rightarrow CO_2+H_2 \text{ and } 4H_2+CO_2 \rightarrow CH_4+2H_2O$$

Krypton–85 concentration (2.8μCi/ft³) NTP (normal temperature and pres-
sure) remained essentially constant over the entire sampling period implying
mixing with a constant volume (1.2×10^8 ft³ NTP) of noncondensable gas during
this time.

[1] Work performed under the auspices of the U.S. Atomic Energy Commission. This paper was presented at the Society of Petroleum Engineers Meeting in Houston, Tex., on September 30, 1968.

[2] Dr. Smith and Dr. Momyer are associated with the Lawrence Radiation Laboratory, University of California, Livermore, Calif.

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Tritium was observed primarily as hydrogen gas soon after detonations. A
rapid decrease in HT came within the first month converting most of the HT
to HTO but producing some CH_3T and C_2H_5T. The predominant tritium-containing
species, except at very early times, is CH_3T, at a concentration of 12 to 14μCi/ft³
NTP. Both CH_3T and C_2H_5T concentrations increase slightly over the first 200
days, that of HT continues to decrease. The ratio CH_3T/CH_4 and C_2H_5T/C_2H_6 are
essentially constant over the period from 30 to 200 days implying that the ex-
change equilibrium was attained rapidly. The ratio HT/H_2 continues to decrease
over the same period implying a continuing influx of non-tritiated water into the
chimney and a reasonably rapid exchange reaction between HT and H_2O.

Changes in concentrations of cavity gas components as a function of flow rate
indicate that removal of 30 percent of the original chimney gas was accomplished
by flaring 5×10^7 ft³ at a rate of 5×10^6 ft³/day. This result is encouraging but the
test was too short to provide verification of this process as a reasonable method
of reducing contamination levels.

Among the more important problems related to the application of nuclear
explosives to stimulation of natural gas fields is that of radioactive contamination
of the gas in the chimney formed by the detonation. One of the primary objec-
tives of Project Gasbuggy is to determine the gas quality with regard to con-
tamination by radioactivity and to evaluate various techniques suggested for
reducing this contamination.

A large quantity of data has been collected from analysis of the Gasbuggy
chimney gas, and some systematic trends have been observed. Although some of
the chemical and radiochemical analyses are not yet complete, that portion relat-
ing to the most important nuclides, tritium and krypton–85, can be presented,
along with the mass spectrometric analyses for the major components of the gas.

Only a tentative assessment of the incomplete results is intended by this pres-
entation. More information must be assembled before detailed interpretations
of the chemistry can be made. Indeed, the lack of samples at early times when
major changes were occurring may cause ambiguous interpretation of the proc-
esses involved as far as this particular experiment is concerned.

Analytical procedures

It might be of some interest to describe briefly the process by which the data are
obtained. The sample is introduced to the separation system and condensed on a
large activated charcoal column (figure 1). These separation systems are, in
reality, large-scale gas chromatographs. Samples of Gas-buggy gas as large as
½ft³ can be easily separated, however, the typical sample size is 1 liter. Helium
is passed through the column as the temperature is progressively raised stepwise.
The gases pass through the column in inverse order of their degree of adsorption.
For a complete separation, both charcoal and a molecular sieve are employed at
temperatures ranging from liquid nitrogen (−240°F) to +600°F.

During the course of an elution, the procedure is monitored using a thermal
conductivity detector and an ionization chamber. These are read out on the
chart recorder. The purified gas is recovered for transfer to a radiation counter.

Two types of counters are used. The "gas-cell," thin-window proportional
counter is employed for krypton-85 radioassay. The krypton is contained in small
cells that are loaded on a sample changer. Each cell, in turn, is rotated underneath
the lead shield of the counter, where it is raised to the counting position. Gases
containing tritium are placed in internal proportional counting tubes and become
a part of the fill gas in the active volume of the tub. These tubes are counted inside
a shielded cave.

Systematic errors which may be present in these determinations are not esti-
mated. The calibration factors for the two counting methods are known to within
a few percent and were determined by counting gases of known radioactivity. A
conservative estimate of the uncertainty in the absolute value of these measure-
ments is, therefore, less than ±10 percent of the value given. Precision of these
determinations is improved by our standard practice of counting replicates.
Results of duplicate counts are averaged to obtain the final result and an estimate
of its reliability. The numbers to be presented here have individual standard
deviations of less than 3 percent. Precision within a group of samples includes
this uncertainty but is primarily determined by real variations in sample composi-
tion between samples. As will be seen, this variation is signficant for hydrogen but
much less so for the other gases of interest. In the data which follow, the precision
of the measurements is indicated by inclusion of ±1 sigma (standard deviation

of the mean) as the indicated uncertainty. In the plotted data, error bas are used to indicate precision, standard deviation of the mean included. Where no such bars are shown, the error bars lie within the plotted point system. All data are related to cavity gas after air (based on O_2) was removed from the sample.

Analytical errors vary according to the percent composition, but are generally less than 1 or 2 percent for the species of interest here. Small variations between samples within a group have been observed and are the primary source of the errors attached to the data.

Sampling

At the present time, data are available from 16 samples grouped in five sampling periods. Except for the production testing which occurred last June and July, no significant variation was observed within a sampling period. Therefore samples within a period have been averaged. Only these averages are presented. These averages are identified according to the mid-point of their sampling time following the detonation (December 10, 1967) as follows:

1st day samples: Four samples were obtained as a result of leakage through the cable conduits to the sealed annulus of the emplacement hole. While these samples were gathered about 1 day after the detonation, the actual time of their separation from the body of chimney gas is most certainly much shorter. They probably represent the chimney gas composition shortly after chimney collapse. Two were suitable for radiochemical analysis. However, these were 85 percent air. Therefore, the errors of the chemical analyses are magnified for the 15 percent of the sample deemed "cavity gas". In spite of this, the results do seem to provide useful information and to fit well with the main body of information obtained from the other groups of samples.

34th day samples: Seven samples were taken between 32 and 36 days after detonation when communication with the chimney by GB–ER [a] had been established. Five of these have been analyzed—two downhole samples and a surface sample taken before 3×10^5 ft³ of gas was flared and one downhole and one surface sample after flaring. No significant differences in the results reported were seen between surface and downhole samples, or between samples prior to and following the flaring. The air correction of sample composition to obtain cavity gas composition was a few percent for these samples.

79th day samples: Of the four samples taken, results from one surface and one downhole sample following flaring of 4×10^4 ft³ of gas are reported. As was the case of 34 days, no significant difference between samples was observed. Downhole sampling has therefore been eliminated. The air correction was only 1 percent for these samples.

134th day samples: Results of the two surface samples taken after 4×10^3 ft³ of gas were flared are included in this report. No air correction was required.

203rd day samples: Data points at 203 days are taken from the first good sample obtained during the extended flow test. Approximately 5×10^6 ft³ of gas had been flared prior to sampling. Samples were taken at intervals of 5×10^6 ft³ or daily. Analytical results from six of these will be reported. No air correction was required for these samples.

Gasbuggy analytical results

For convenience and presentation the samples have been divided into two time periods. The first group is composed of those samples taken during the shut-in period prior to flow testing. Samples obtained during the flow testing comprise the second group. Somewhat arbitrary curves have been drawn through the data points.

Figure 5 (table 1) presents the observed changes in chemical composition of the cavity gas as a function of time. The effect of temperature equilibrium is clearly evident. Light gases predominate at early times, moving towards more complex gases as the chemical equilibrium shifts. Plotted across the lower portion of the graph is the total volume of gas with which the krypton–85 is mixed. Its constancy within analytical uncertainty is remarkable. Evidently equilibration of the cavity with formation pressure occurred quite rapidly and has been maintained throughout the shut-in period.

The total gas volume was obtained by dividing the total krypton–85 by the krypton–85/ft³ NTP (normal temperature and pressure) determined from radio-

[a] GB–ER, Gasbuggy-Emplacement re-entry hole, a post-detonation hole drilled directly through the original emplacement hole.

chemical analysis of the samples. Total krypton–85 is estimated at 350 curies according to the anticipated performance of the nuclear explosive (*1*).

The rapid increase in CO_2 and the corresponding decrease in CO suggests that the water-gas reaction:

$$CO + H_2O \rightarrow CO_2 + H_2 \qquad (1)$$

reaches equilibrium at early times.

The gradual decrease in H_2 concentration appears to be due to the reaction:

$$4H_2 + CO_2 \rightarrow CH_4 + 2H_2O \qquad (2)$$

which is observed to proceed slowly toward equilibrium throughout the sampling period. In addition, natural gas from the formation has entered the cavity to maintain constant pressure. Adding reactions (1) and (2) produces:

$$3H_2 + CO \rightarrow CH_4 + H_2O \qquad (3)$$

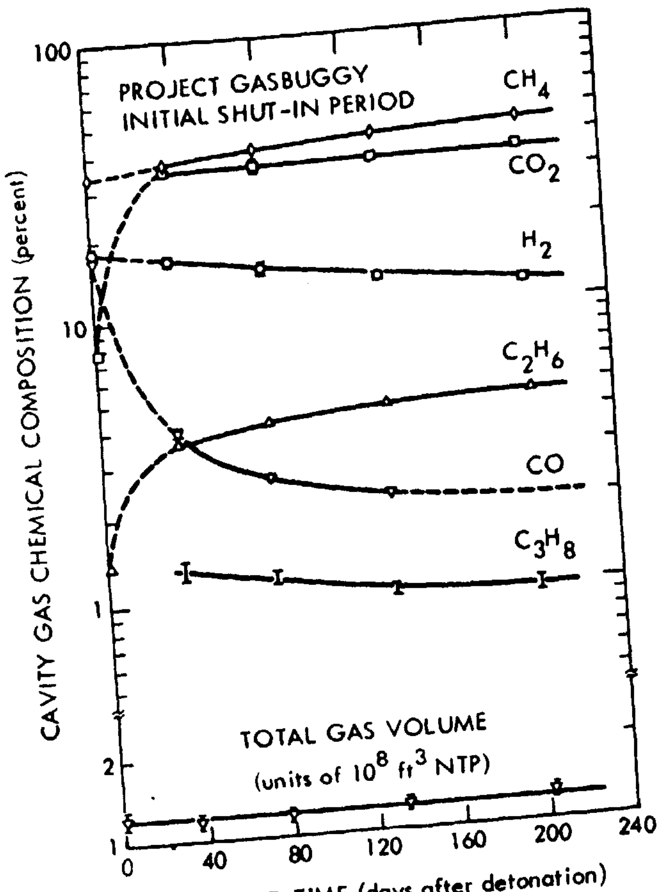

Figure 5. Chemical composition of cavity gas as a function of time after detonation

Note that CO_2 does not enter and is indeed constant over the major samping period. H_2 and CO and being used up while methane is increasing. The observed increase in ethane concentration may be due to a reaction such as:

$$CH_4 + CO + 2H_2 \rightarrow C_2H_6 + H_2O \qquad (4)$$

The observed decrease in propane may or may not be significant. Variation such as that seen can be attributed to fractionation of the sample during the later sampling periods.

Another way to view the chemical data is in terms of totals of elements in the gas (figure 6). The total gas volume is plotted across the bottom of the figure to provide a base line. Above it, in the center of the figure are the concentrations of the elements of interest. Because of the constancy of the total gas volume, these curves represent totals equally well. The curves at the top of the figure are chemical composition in terms of atom percent.

This figure illustrates the trend toward more complex molecules by the chemical reaction processes coupled with the influx of formation gases to maintain a constant volume of gas within the chimney. Note that the fraction of hydrogen decreases even though the total number of hydrogen atoms increases early.

The increase in oxygen at early time is due to the production of CO_2 by the water-gas reaction (1). The decrease at late time can be accounted for by invoking reaction (3).

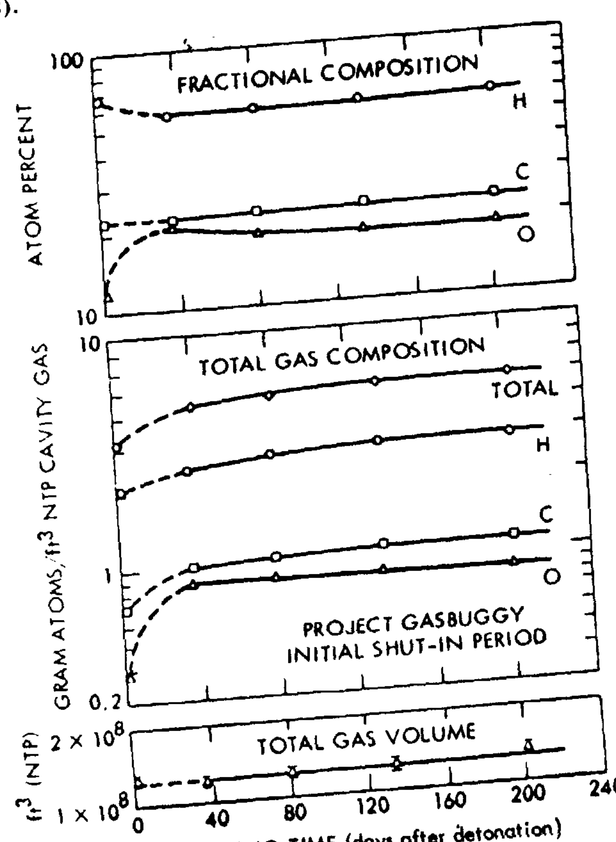

Figure 6. Elemental composition of cavity gas in a function of time after detonation

TABLE 1.—CHEMICAL COMPOSITION OF GASBUGGY CAVITY GASES

Sampling time (days after detonation)	Mole percent					
	H_2	CH_4	C_2H_6	C_3H_8	CO	CO_2
1	18.1±0.6	34.0±0.5	1.37±0.02	....	17.1 ±0.4	7.8±0.2
36	16.8± .4	36.9± .8	3.60± .06	1.3 ±0.1	3.9 ± .2	35.8± .5
79	15.2±1.0	40.1±2.0	4.1 ± .2	1.19± .04	2.6 ± .1	35.8±1.6
134	13.2± .1	43.2± .7	4.5 ± .1	1.02± .05	2.20± .04	35.3± .7
203	12.0± .2	44.2± .7	4.7 ± .2	.97± .05	....	36.2± .7

The radiochemical results for tritium in hydrogen, methane, and ethane, are plotted on figure 7 (table 2). Again, total gas volume is plotted along the base line for reference. The observed gas volume of 1.2×10^8 ft^3 NTP can be contained in $2.1 \pm 0.1 \times 10^6$ ft^3 void at 150° F and 950 psig[4] (the observed conditions on January 23). Such a void was estimated from data obtained during the production testing, implying that the total krypton-85 estimate is reasonable.

The actual krypton-85 data seem to be moving toward lower concentrations but because of analytical errors associated with the data no conclusion can be reached as to the significance of this trend. The best fit to the data within these uncertainties is probably the line shown.

At early times a large fraction of the gaseous tritium existed in the form of hydrogen gas. The data point for gaseous tritium at 1 day corresponds to some 30 percent of the total of 4 grams of tritium assumed present in the post-shot chimney. During the first month the HT level dropped quite rapidly and continued to decline at a slower rate. Prior to the time re-entry of the chimney well was accomplished, tritiated methane became the principle contaminant of the chimney gas. About 5 percent of the total tritium remains gaseous at late times. Presumably the other 95 percent is in the form of water. No meaningful tritiated water results can be reported. Obtaining a representative sample of water in the chimney gas is extremely difficult. The actual numbers range from about a microcurie per cubic foot of cavity gas to 0.001 of that value, the variation being due primarily to dilution of the tritiated water in the sample by tritium-free water within the cavity and re-entry well casing coming from the overlying aquifers.

[4] psig—pounds per square inch gauge.

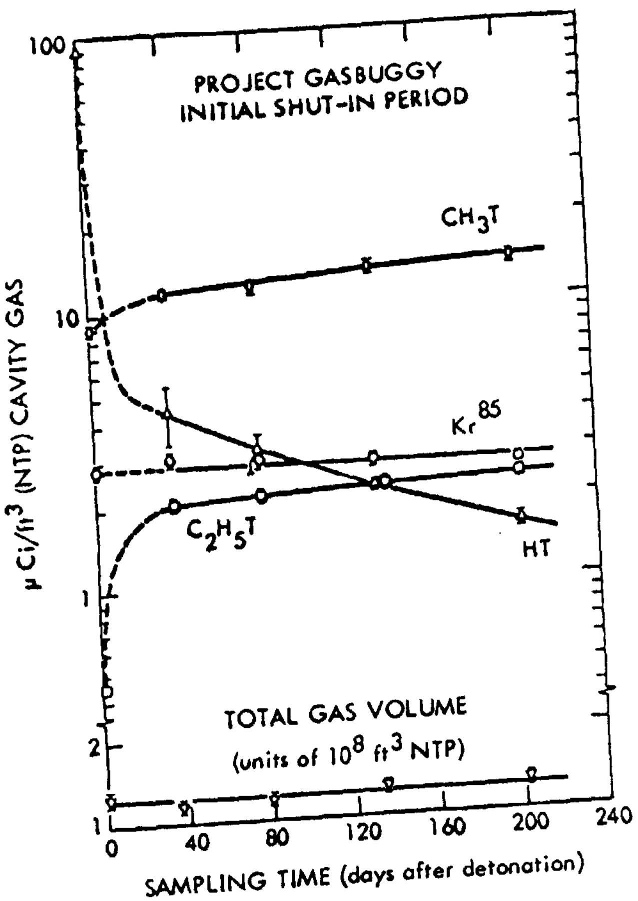

Figure 7. Radionuclide concentrations in cavity gas as a function of time after detonation

TABLE 2.—RADIONUCLIDE CONCENTRATION IN GASBUGGY CAVITY GASES

Sampling time (days after detonation)	Total gas volume (10^8 ft³ NTP)	Radionuclide concentration [1] (μCi/ft³ of cavity gas)				
		Krypton-85	HT	CH_3T	C_2H_5T	Total tritium
1	1.25±0.08	2.8±0.2	91.0±1.0	8.9±0.1	0.45±0.01	100±1.0
36	1.17± .07	3.0± .2	4.4±1.1	11.7± .1	2.06± .09	18±1.0
79	1.20± .08	2.9± .2	3.1± .5	12.0± .7	2.15± .06	17±1.0
134	1.24± .06	2.8± .1	2.3± .1	13.6± .7	2.3 ± .1	18±1.0
203	1.28± .07	2.7± .1	1.6± .1	13.9± .7	2.4 ± .1	18±1.0
Average	1.23±0.04	2.8±0.1				[2] (18± .8)

[1] Errors quoted are 1-standard deviation of the mean of averaged measurements.
[2] Average does not include the 1 day sample result.

Iodine-131, a potential problem radionuclide at early times, was not seen in any of the samples. An upper limit of 10^{-3} μCi/ft³ (NTP) of this radionuclide existing as a gas within the chimney appears conservative. No other radionuclides have been detected which would cause a problem at times longer than a few months. Argon-37 produced by neutron activation of the calcium in the rock is the only other radionuclide now prominent in the gas. Its initial concentration was about 120 μCi/ft³ NTP. Due to its half-life (35.4 days) the argon-37 concentration is now less than that of krypton-85.

Analysis of a sample obtained from GB-2R [a] is not yet complete, but does indicate the presence of a small quantity of cavity gas in the formation out to at least 300 feet.

Changes in the concentrations of the tritiated species appear to follow the trends observed for chemical compositions, with the exception that the decrease of tritiated hydrogen is more marked than the corresponding decrease in hydrogen gas. Equilibrium reaction such as the water-gas reaction (1) provide a path whereby tritium can exchange with hydrogen in water, reducing the tritium concentration. The overall effect is that the ratio of tritium to hydrogen tends to equalize in all hydrogen containing species participating in the exchange. The degree to which this is observed depends on the exchange rate.

The rapid initial decrease in HT concentration can therefore be explained using the water-gas reaction:

$$HT+CO_2\rightarrow HTO+CO \tag{5}$$

and reactions such as (2) and (4) can be used to explain the observed increases in tritiated methane and ethane.

In figure 8, specific activities are compared for the tritiated species. These curves demonstrate the trend toward a uniform tritium to protium ratio. The HT/H_2 ratio is seen to drop quite rapidly as the water-gas reaction approaches equilibrium and to change slope as the slower chemical reactions begin to dominate. The rapid decrease observed indicates that the T/H in cavity water was quite low, and is consistent with the production of CH_4 and C_2H_6 by reactions such as (2) or (4).

The fact that both the CH_3T and C_2H_5T curves appear to be flat over the entire sampling period is not contradictory. They are being produced by hydrogen of nearly comparable specific activity. In fact, the samples at 134 days show very nearly the classical 1 : 2 : 3 ratio for the three curves. Presumably the eventual downward trend should appear when the HT is further reduced by exchange with water. The fact that exchange equilibrium existed at 134 days and that the HT/H_2 ratio is still decreasing may imply entry of non-tritiated water into the chimney.

[a] Second Gasbuggy re-entry hole drilled about 300 feet from GB-ER.

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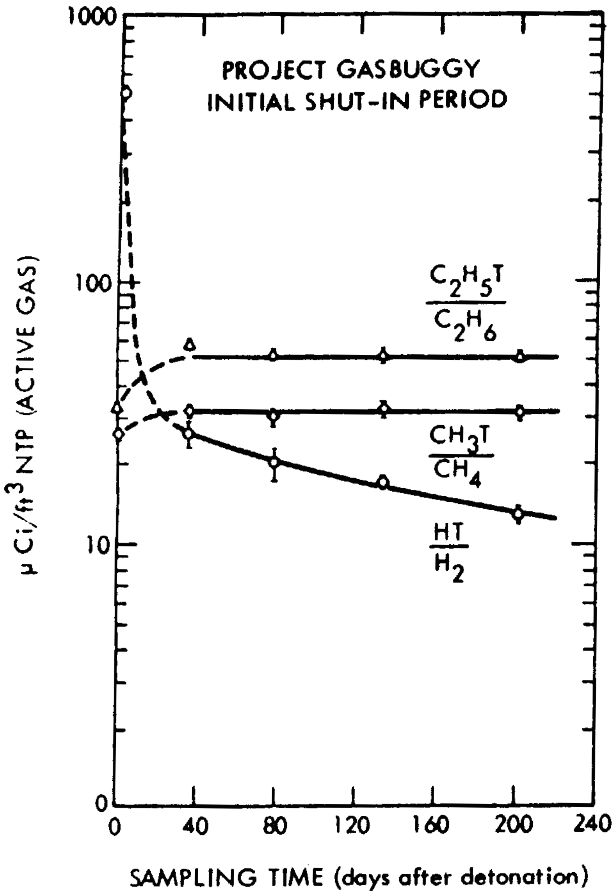

Figure 8. Specific activities of tritiated species as a function of time

Isotopic effects are not considered here but would also tend to reduce the tritium as elemental hydrogen in favor of water at low temperatures.

Changes in chemical composition of the gas during the 11-day flow test which occurred during June–July 1968 are plotted on figure 9. Logarithmic increases with flow for components of natural gas are complimented by corresponding decreases for gases solely of cavity origin. Results plotted cover the flaring period at 5×10^6 ft³ per day nominal flow.

The produced gas is nearly like cavity gas in composition. About one third of the original cavity gas was removed by dumping two fifths of a cavity volume of gas. On the average only 17 percent of the produced gas came from outside the chimney. At this rate a factor of 10 reduction in contamination of the chimney gas can be achieved by flaring about two chimney volumes (2.5×10^8 ft³ NTP). Further experiments are needed to define the long-term behavior of the cavity flushing and to establish a consistent model for estimating the fraction of cavity gas removed as a function of flow rate.

Radiochemical analysis of these samples are not yet complete. Preliminary assessment of changes in concentration that have been observed, do, however, seem to generally fit those shown here for CO_2 and H_2. They do, however, show a marked deviation from the lines established at the higher flow indicating much

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more dilution of the cavity gas by influx of formation gas. This observation corresponds to the observed increase in cavity pressure during the low-flow rate flaring.

The gas quality program at Lawrence Radiation Laboratory is continuing its investigations in an effort to gain a better understanding of the complex interactions of the Gasbuggy gas with itself and its environment. These results, and the interpretations which can be drawn from them, will be publicly available in the future.

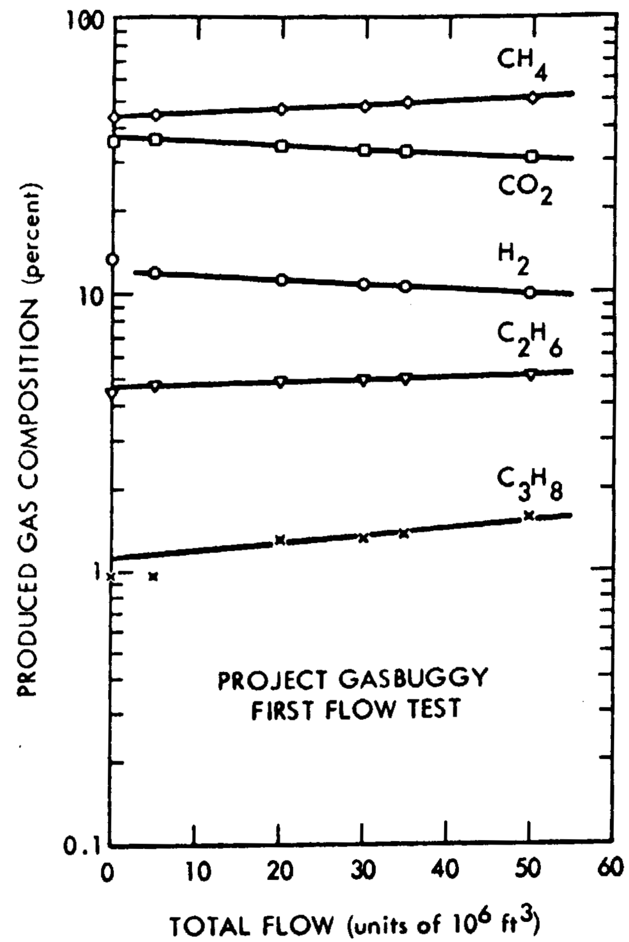

Figure 9. Changes in chemical composition of gas during 11-day flow test in June–July 1968

This is precisely what one would expect on the basis of its known properties. When present as hydrogen gas initially, tritium will exchange readily with organic molecules. So this is what one would expect.

The real issue is to what extent does this radioactive gas then subsequently change back as the tritium is carried away. The implication of the July report is that the tritium is being carried away in ground

water, where, I don't know. In the process the amount of tritium in hydrogen gas molecules is decreasing. By that kind of study one could predict when the level of tritium was low enough so that you could go ahead and use the hydrocarbons commercially. At the present time the tritium content of methane is quite high.

They looked for iodine-131 in the gas phase and didn't find any. Krypton-85 was present, as well as some argon-37. I think the point here, and really the basis of your question is: Is the information complete?

In other words, are we being told the whole truth? This question raises some very important issues. I think that there isn't any question that the Atomic Energy Commission has done its level best to get all of the information to any people who were interested when they wanted it.

The mass of information is so enormous and so detailed that no one single individual could possibly encompass it all without a rather large staff to make sure that all the reports were in hand.

So there is an information gap that has nothing to do with the intent of the people providing the information. They are not intending to withhold information.

The other aspect of it is that the information may not be complete and that may apply in the case of Gasbuggy. There is another problem when these issues take on an emotional component and I am afraid in many cases emotion enters the picture. For example, the emotions that will arise in the minds, I am sure, of those people who have had to do with setting of standards, when they hear the results of this testimony. They are going to be emotionally affected. Everyone would be. They are being challenged. When this emotional milieu sets in, then a lot of very subtle psychological bias can be given range.

For example, the question of the significant genetic dose came up in a discussion that we had with representatives of the Atomic Energy Commission in October, arising out of our concerns about the Calvert Cliffs reactor. We had a 2-hour meeting and yet it turned out there were still points where we did not agree whether we agreed, if you follow me.

In other words, we were asked to draw up a list of subjects we all agreed on or thought we did, and then a list of subjects that we didn't agree on after the discussion. It turned out that there were important areas in the list that we thought we agreed on that we didn't.

Now, I can only conclude that one of the reasons this did not come out was that there was an emotional kind of adversary situation in that particular meeting, unfortunately. If it had been open, scientifically, with no emotions involved, then presumably we would have raised this particular point which has to do with the question of the threshold, the one I mentioned earlier. We didn't get that information exchanged and I am really dumbfounded to find that the Atomic Energy Commission representatives are taking the position of there being a threshold, because I didn't get any inkling of it from the meeting.

If we had, we probably would have spent the whole meeting discussing the scientific basis for that attitude because it is very crucial. So, in answer to the question of information being incomplete or something like that, we have first the question whether it has been gathered adequately, and here a lot of biases enter into it, too.

For instance, iodine-131 was not measured in fallout for many years after the first atmospheric tests began because it had been felt to be of such short life that it couldn't be of any importance biologically.

Therefore, we did not need to bother to measure it. It was found out later, to their regret that it was important. Thus, it turns out that certain kind of mind sets can affect what information you gather in the first place.

Secondly, when you do gather it, it is tremendously difficult to get it distributed to people who want to use it.

Thirdly, once you get into a controversy state, then you have to watch out for the subtle emotional reactions that we all are heir to, and that I am heir to, just as much as anyone.

Senator MUSKIE. I notice on the editorial advisers of this publication of PHS *Radiological Health Data and Reports* there are representatives of the Department of Defense, Interior, Agriculture, Commerce, Food and Drug Administration, and Atomic Energy Commission.

Now are these reports truly independent of AEC policy or AEC control in your judgment?

Dr. RADFORD. Well as far as I am aware they are certainly free of AEC control. I am not familiar with the detailed editorial policy of this publication. Inevitably, there grows up in an organization a certain kind of policy.

For example, a number of years ago Dr. Vilma Hunt and I proposed that radioactive polonium in tobacco may have some relation to cancer arising from cigarettes. Now, this opened the issue of the effect of very low doses of alpha radiation, if I can be that technical, on the epithelium of the respiratory tract, that is the lining of the respiratory tract.

Clearly, if the radioactive material polonium does play some role in lung cancer, then this has great implications for the atomic energy program. It suggests that alpha emitters may be quite hazardous when present in low amounts in the environment.

My scientific position has always been that either way it is an important answer we will get. That is, if polonium is not contributing to lung cancer in smokers, this is an important observation.

Now, the reason I tell this story is that immediately one could sense the reaction of a large segment of the scientific community to this suggestion. Most of the people in the atomic energy program immediately dismissed polonium out of hand. There are reasons why it may not be important.

That is, our hypothesis is not absolutely certain. Far from it, and we would be the first to say so. But the alacrity with which it was dismissed out of hand indicated that those individuals had a very definite bias.

Now, some of the people writing these reports in this document have been very closely associated with the atomic energy program and I would be very surprised indeed if they didn't have a certain attitude toward it. That is just human nature and it is not going to change.

Senator MUSKIE. Let me read you something from this report. It troubles me because of its tone. This is with respect to description of Project "Gasbuggy." I quote:

Nevertheless the AEC hypothesis had all possible failure modes which could release radioactivity into the atmosphere, into ground water, or into the natural gas production system, although these failure modes were considered highly unlikely.

Now, if this is bias on which the safety program is established; that is, that these failure modes were considered highly unlikely, would the safety program truly focus effectively upon the risks?

It seems to me this suggests less than full sensitivity to the risk that one ought to take or one ought to have when beginning a first project of this kind.

Dr. RADFORD. Well, I really can't add a great deal to what you have said, Senator. I am sure the men were motivated by the proper concern. I think the kind of question you have just asked is precisely the kind of question that any agency that is responsible for the safety and development both, is wide open, to.

And they don't have a good answer for it any more than I have a good answer, except to say that your concern is valid.

Senator MUSKIE. I have one other point to make with respect to this report. Let me read some of the conclusion on page 23:

The first experiment involving the industrial application of the peaceful use of nuclear explosives resulted in no release of radioactive material into the environment at the time of detonation.

The second conclusion:

Initial gas samples collected from the producing wells within approximately a 5-mile radius of ground zero, as well as sampling of the El Paso Natural Gas Co.'s distribution system, have verified that no migration of radionuclides into other wells has occurred.

Then the final conclusion:

Project "Gasbuggy" represented no hazard from radioactivity to the population in the vicinity of the detonation site nor has it resulted in the introduction of any device-related radioactivity into the existing natural gas distribution system of the El Paso Natural Gas Co.

Again, in that summary there is no reference to the effect upon the gas in the well which was developed by the explosion. It seems to me that if there was no examination of the results, that should have have been stated, or if the results were unfavorable, that should have been stated.

The report simply omitted discussion of that aspect of the explosion.

Dr. RADFORD. You are reading from the July 1969 report?

Senator MUSKIE. No. This is the advance report that will appear in the December issue, I am told.

(The report referred to follows:)

1128593

Offsite Radiological Surveillance for Project Gasbuggy June 1967-July 1968

John R. McBride and *Dixon Hill*[1]

Project Gasbuggy, an experiment to stimulate gas recovery by nuclear means, was conducted on December 10, 1967, as part of the Atomic Energy Commission's (AEC) Plowshare Program. The Public Health Service by Memorandum of Understanding with the AEC is responsible for a comprehensive offsite radiological safety program. The data obtained during this program have documented that no radioactivity was introduced into the environment as a result of the Project Gasbuggy detonation. Surveillance of the El Paso natural gas producing wells near the Gasbuggy experiment was conducted to insure that gas contaminated with radioactivity was not present.

On Sunday, December 10, 1967, at 12:30:00 mountain standard time (m.s.t.), at a location 55 air miles east of Farmington, N. Mex., a 26-kiloton thermonuclear explosive was detonated 4,240 feet below the surface of the Carson National Forest (figures 1 and 3). The event, Project Gasbuggy, was the first joint government-industry experiment in the U.S. Atomic Energy Commission's (AEC) Plowshare Program. The joint sponsors, in addition to the AEC, were the U.S. Department of the Interior (Bureau of Mines) and the El Paso Natural Gas Company.

The main objective of the experiment was to determine if nuclear explosions can increase the production rate and ultimate recovery of natural gas from low permeability gas formations. Some flow tests, as well as gas quality analyses, have been made of the Project Gasbuggy well by the project participants; however, until more extensive flow tests are made in 1969, conclusive information will not be available as to the degree of success of the experiment.

Factors affecting the safety of the project included: the depth of emplacement of the device, the proximity of an aquifer to the detonation, and the location of gas production wells with respect to ground zero. The device was placed at a depth of 4,240 feet in the Lewis Shale formation in the San Juan Basin. This depth is about 40 feet below the bottom of the gasbearing Pictured Cliffs formation (figure 2). At this depth, the device was considered to be overburied by safety standards in use at the Nevada Test Site. (A 26 kt device would be considered safely emplaced at NTS at a depth of approximately 1,200 feet.) The lowest water bearing formation is the Ojo Alamo sandstone which is located from 3,475 to 3,650 feet below the surface at the site. The nearest aquifer, therefore, is approximately 590 feet above the shot point. The site chosen for the project is on land leased by El Paso Natural Gas Company. The only wells in the area belong to them and the closest production well is 3,400 feet from ground zero. As a precaution, all producing wells within approximately a 5-mile radius of ground zero were physically separated from the gas-transmission system. In addition the AEC hypothesized all possible failure modes which could release radioactivity into the atmosphere, into ground water, or into the natural gas production system. Although these failure modes were considered highly unlikely, the AEC authorized a comprehensive radiological safety program for Project Gasbuggy.

[1] Mr. John R. McBride is deputy director and Mr. Dixon Hill was project engineer, Southwestern Radiological Health Laboratory, Las Vegas, Nev. During Project Gasbuggy, Mr. McBride was senior PHS official on the Project. Mr. Hill was project officer for Gasbuggy in charge of all offsite radiological safety activities.

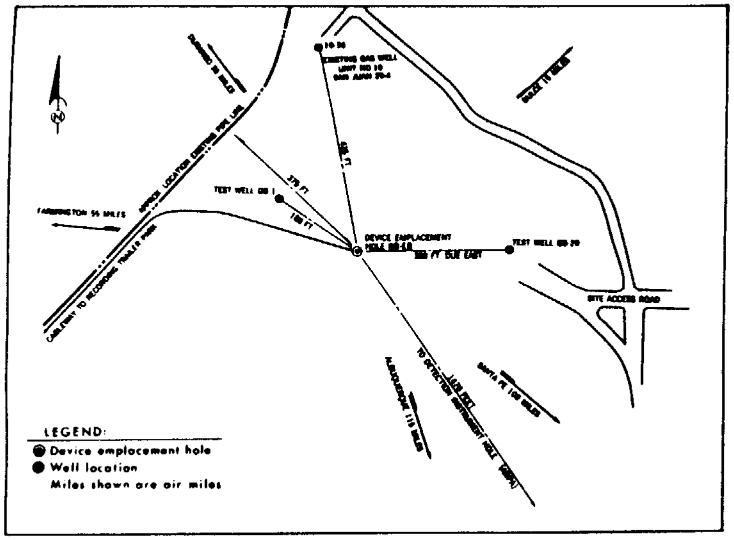

Figure 1. Project Gasbuggy emplacement hole and well locations

Offsite radiological safety program

This article summarizes the offsite radiological safety program for Project Gasbuggy as conducted by the Southwestern Radiological Health Laboratory (SWRHL) of the Bureau of Radiological Health. In accordance with a Memorandum of Understanding between the AEC and the Public Health Service, the SWRHL is responsible for conducting offsite radiological safety programs for all U. S. nuclear tests.

A surveillance program was established by SWRHL to collect and analyze environmental samples before and after the nuclear detonation in order to document any possible release of radioactivity to the offsite area and to be prepared to handle emergency procedures to insure protection of the public health in case an unforeseen accident occurred. Samples provided data on air, milk, water, natural gas, and external radiation levels in the area surrounding the site.

The offsite surveillance program was divided into three periods:

1. The preshot preparations—June 1, 1967, to detonation (1230 m.s.t. December 10, 1967);
2. The post-shot period, including the redrill of the emplacement hole (GB-ER)—December 10, 1967, to January 19, 1968;
3. The redrill of the Gasbuggy-2 (GB-2R, one of the two exploratory wells drilled before the emplacement hole) and subsequent natural gas flaring operations from GB-ER and GB-2R—June 13 to July 22, 1968.

Environmental samples were collected preshot to establish background radioactivity levels. Following the completion of the post-shot period and again after the GB-2R redrill period, samples were collected to form a comparison with the background data.

The SWRHL preshot preparations in the Project Gasbuggy area began in June 1967. During the summer of 1967, a census was taken of all people and milk cows within 100 miles of the Gasbuggy site; in addition, all mining and tunneling operations within 50 miles were located.

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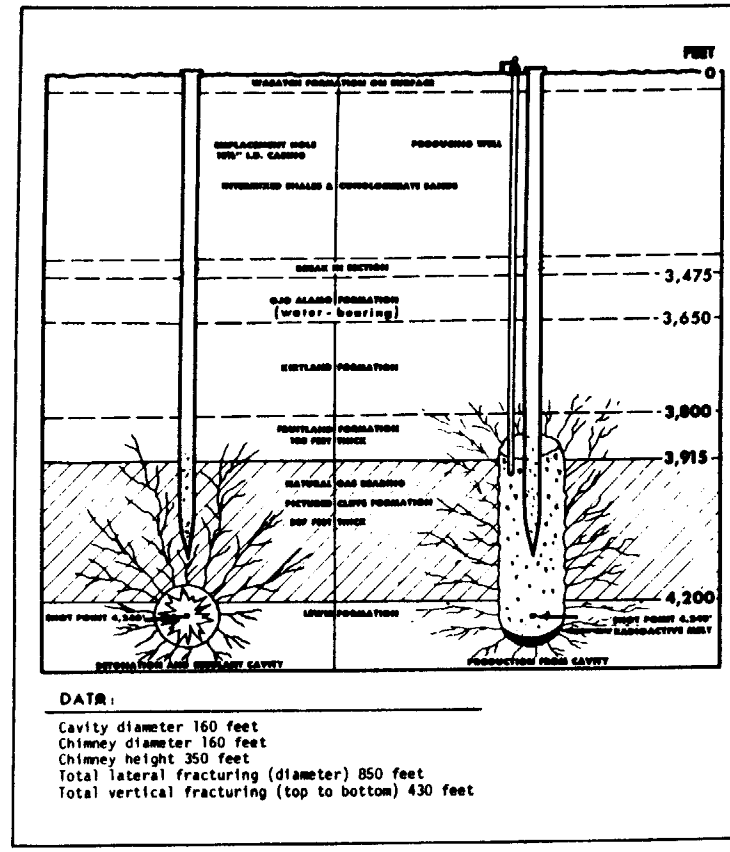

Figure 2. Project Gasbuggy predicted underground effects

As the census information was collected, personnel distributed printed information explaining the nature of the experiment and answered questions regarding their activities. The Community Relations Program was intensified during later periods when the SWRHL project officer and State health department officials visited local officials in surrounding communities. The initial environmental sampling was begun in August 1967. The dosimetry program began in October 1967. Medical and veterinarian activities began during the preshot period when the respective officers made visits to various State and local officials; these activities continued throughout the drill-back of GB-ER. Approximately 30 people from the SWRHL and the health departments of New Mexico and Colorado were assigned to the SWRHL Gasbuggy surveillance program on December 1, 1967. A short training course was given to State personnel on procedures to be used and all personnel were oriented to the area around the site.

At shot time, SWRHL had 33 people on station, including monitoring teams in two aircraft circling the site. The number of people was reduced in mid-December to five who remained throughout the redrill of GB-ER.

The GB-2R redrill operations began in mid-June 1968, and were completed by mid-July 1968. They included the redrill of GB-2R and flow testing (flaring) of natural gas from the GB-ER well for 15 days. The project officer was onsite for these operations. Staff previously assigned to the project were on standby at SWRHL in the event additional field personnel were needed.

Air surveillance

The Project Gasbuggy Air Surveillance Network collected daily samples preshot and during the post-shot period at 35 locations surrounding the Gasbuggy site. Six of these stations were part of the SWRHL Air Surveillance Network (ASN) and two were activated ASN standby stations. The remaining 27 stations were established specifically for the project. The Gasbuggy network is shown in figure 3.

Air sampling stations were equipped with Gelman Tempest air samplers using a Gast Model-1550 positive displacement pump. The filter system used a 4-inch diameter Whatman-541 filter in series with a 4-inch MSA[1] activated charcoal cartridge.

The Gasbuggy Surveillance Network station began operations on November 27, 1967 and continued through December 13, 1967. The nine stations nearest the site were operated throughout the GB-ER drill-back period until January 19, 1968. A total of 1,120 sets of samples was collected. In mid-April 1968, 16 of the 27 special stations were discontinued, leaving 11 on standby. These stations were operated from June 30 to July 18, 1968. A total of 200 sets of samples was collected during this period.

All filters and charcoal cartridges were mailed directly to SWRHL for analysis. The filters were analyzed for beta radioactivity upon arrival at the laboratory and again on the fifth and twelfth day after collection. If the initial beta-particle count indicated a radioactivity concentration of more than 1 picocurie per cubic meter, or if the 5-day count was more than 150 counts per minute above background, the filter was analyzed by gamma-ray spectroscopy. The charcoal cartridge was gross gamma-ray counted upon arrival and, if the count was greater than 500 counts per minute, the filter and cartridge were analyzed by gamma-ray spectroscopy. These control limits were based on normal background concentrations.

The air sampling results for each of the three periods are summarized in table 1. The post-shot period is further divided into two periods since levels of radioactivity in air were generally higher across the United States during the last 5 days of December 1967.[2] Examples of this increase at scattered stations west of the Mississippi River are given in table 1 (footnote c).

Milk surveillance

Milk sampling coverage was provided by the Gasbuggy Milk Surveillance Network during the preshot and operational periods. Twenty-two stations were sampled; 13 represented family milk cows and nine were Grade-A dairies. The station locations are shown in figure 4.

Samples were collected at each location during the following periods: July 30 to August 1, 1967; September 4-7, 1967; October 20-25, 1967; and January 19-20, 1968. In addition, five samples were collected on December 14, 1967. A total of 75 1-gallon samples was collected and shipped to SWRHL for analysis.

The analysis of the 1-gallon milk samples consisted of two procedures: gamma-ray counting and radiochemical analysis for strontium. Upon arrival at SWRHL, 3½ liters of the sample were analyzed by gamma-ray spectroscopy for 40 minutes. The gamma-ray analysis information was proc-

[1] MSA—Mine Safety Appliance, part 46727 charcoal cartridge. The cartridge is an organic-vapor-type cartridge containing stable iodine to improve the organic iodide retention capability.

[2] Additional information on increased levels can be found in reference 1.

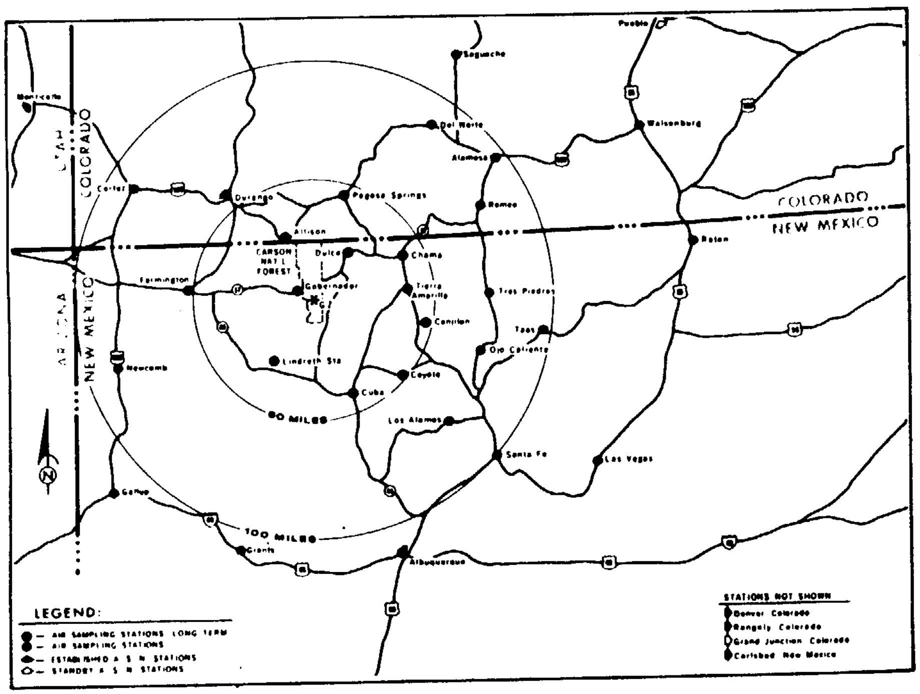

Figure 3. Project Gasbuggy air surveillance network

Table 1. Summary of beta radioactivity in air[a] Gasbuggy air surveillance network

Period[b]	Number of stations operating	Number of samples taken	Number of samples above detectable limits	Range (pCi/m³) Minimum	Range (pCi/m³) Maximum	Average of samples above detectable limits (pCi/m³)	Average of all samples (pCi/m³)
Period 1—preshot: 11/27/67 to 12/9/67	35	404	80	0.1	1.3	0.2	[c]<0.1
Period 2—post-shot: 12/10/67 to 12/24/67	[d]35 or 12	240	27	.1	.3	.1	<.1
12/25/67 to 1/19/68	[e]12 or 25	371	336	.1	7.8	1.0	.9
Selected ASN stations:[f] 12/25/67 to 1/19/68	5	106	63	.1	9.5	.8	.5
Period 3—GB-2R redrill 6/13/68 to 7/18/68	10	200	190	.1	1.0	.2	.2

[a] Gamma-ray analysis results, none of which showed radioactivity above background, are available from SWRHL.
[b] 1/19/68 to 6/13/68—all stations on standby.
[c] Detectable limit: beta radioactivity ~0.1 pCi/m³ based on 300 m³ and 2-minute-counting time.
[d] 12/10/67 to 12/13/67: 35 stations operated. 12/13/67 to 12/24/67: 12 stations operated.
[e] 12/12/67 to 1/11/68: 12 stations operated. 1/11/68 to 1/19/68: 25 stations operated.
[f] Selected ASN stations:

Station	Station no.
Minneapolis	[illegible]
Berkeley	[illegible]
Seattle	50
Phoenix	43
New Orleans	92

These scattered stations were selected to show that the increased radioactivity was widespread.

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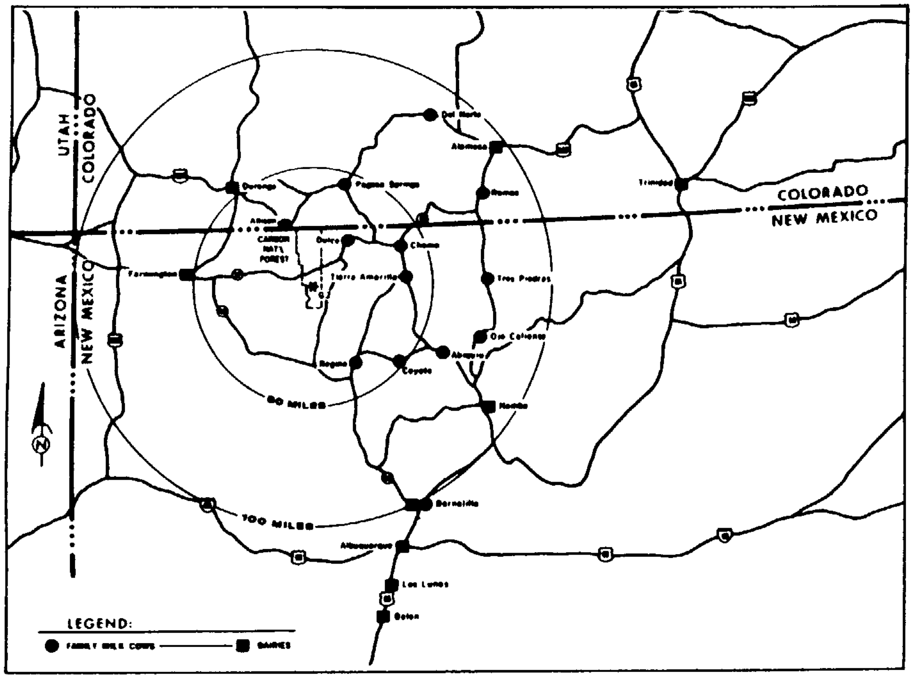

Figure 4. Project Gasbuggy milk surveillance network

cessed through a computer program which quantified the samples for iodine-131, cesium-137, barium-140, and lanthanum 140. After the computer program completed the results, the spectroscopic information was examined to ascertain that no other radionuclides were present. The strontium analysis consisted of passing 1 liter of the sample through an ion-exchange resin column which retained the strontium. The strontium, after being eluted from the column, was precipitated as a carbonate compound and was counted after weighing and again 1 week later. The strontium-89 and -90 concentrations were solved by simultaneous equations.

The milk sampling results for periods 1 and 2 are summarized in table 2. No milk samples were taken following the GB-2R redrill (period 3) since all radioiodines had decayed below significant levels.

Water surveillance

Water sampling coverage during the preshot and post-shot periods was provided by a network of 34 sampling stations, shown in figure 5. Fifteen of the sampling locations were within 20 miles of ground zero at open- and well-water sources, and 13 of the locations were the same as the family milk cow sampling stations. Six of the network locations were municipal water systems located within approximately 120 miles from ground zero.

Seventy-four 1-gallon samples were collected on the same schedule as milk samples prior to the GB-2R redrill. Seven samples were collected during and immediately following the GB-2R redrill. All samples were sent to SWRHL for analysis. Upon arrival at SWRHL, 3½ liters of the sample were analyzed by gamma-ray spectroscopy for

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Table 2. Summary of Gasbuggy milk analysis results, periods 1 and 2

Isotope	Number of samples analyzed	Number of samples above detectable limits[a]	Range (pCi/liter) Minimum	Range (pCi/liter) Maximum	Average of samples above detectable limit (pCi/liter)	Average of all samples (pCi/liter)
Preshot:						
Iodine-131	53	0	—	—	—	ND
Cesium-137	53	6	10	34	20	<10
Strontium-89	30	4	5	5	5	< 5
Strontium-90	30	20	3	18	6	6
Post-shot:						
Iodine-131	23	0	—	—	—	ND
Cesium-137	23	1	16	16	16	<10
Strontium-89	20	2	5	5	5	< 5
Strontium-90	20	18	2	8	5	4

[a] Detectable limits: Iodine-131—10 pCi/liter, based on 3,500-ml sample counted for 40 min.
Cesium-137—10 pCi/liter, based on 3,500-ml sample counted for 40 min.
Strontium-89—5 pCi/liter, based on 1-liter sample counted for 50 min.
Strontium-90—2 pCi/liter, based on 1-liter sample counted for 50 min.

ND, nondetectable.

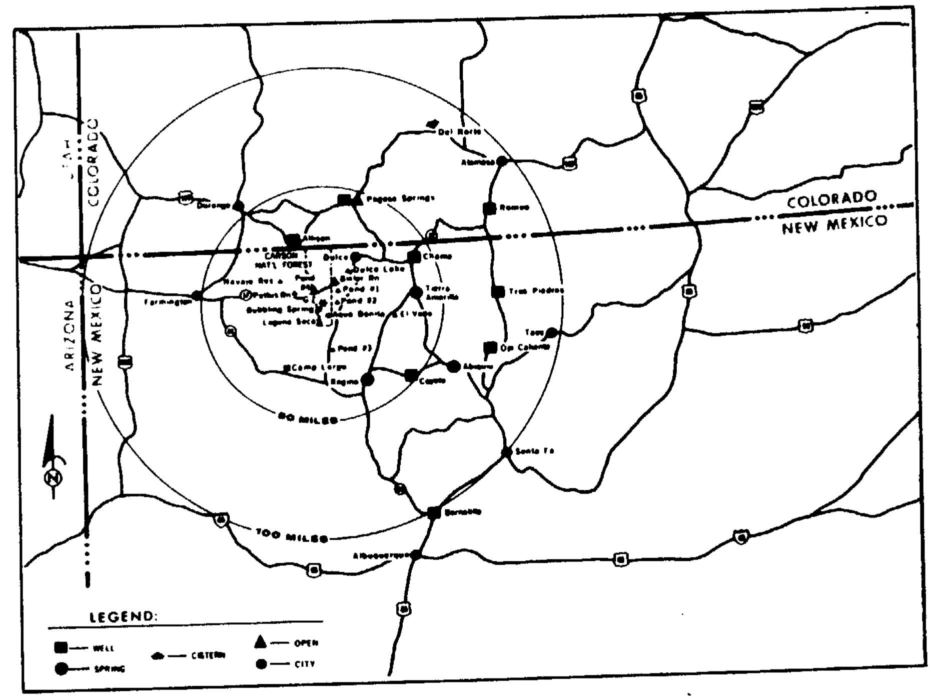

Figure 5. Project Gasbuggy water surveillance network

40 minutes. This information was processed through a computer program which routinely calculates values for the following isotopes; cerium-praseodymium 144, barium-lanthanum 140, cesium-137, iodine-131, ruthenium-106, zirconium-niobium-95, manganese-54, and potassium-40.

If it had been determined that additional analysis for other radionuclides was necessary, a different set of isotopes could have been specified. However, none of the samples required additional analysis. After the gamma-ray analysis, 250 milliliters of the sample were evaporated and the residue counted for gross alpha and gross beta radioactivity. If the gross beta radioactivity had been greater than 100 pCi/liter at the time of counting, a strontium analysis would have been performed; if the gross alpha radioactivity had been greater than 30 pCi/liter, a radium-226 analysis would have been done. None of the samples required these additional analyses.

Water sampling results for periods 1 and 2 are summarized in table 3 and are given in table 4 for period 3. The samples taken during the preshot and post-shot periods (periods 1 and 2) were treated routinely as samples are from all Nevada Test Site operations. The samples taken following the GB-2R redrill (period 3), however, were taken specifically for tritium analysis; beta and alpha-particle counting was not done and all gamma-ray spectra were negligible.

Personnel surveillance

A network of thermoluminescent dosimeters (TLD's) and film badges was established surrounding the Gasbuggy site. The network consisted of 50 stations located at approximately 1 and ½-mile intervals along existing roads and highways, as shown in figure 6.

The background radiation level for the network area was established prior to the shot with stations consisting of three "dedosed" TLD's set out on October 17, 1967, and exchanged on November 15, 1967. They were exchanged again on December 8, 1967, 2 days before the shot, for three "dedosed" TLD's and two film badges.

The TLD's used were EG&G model TL-12 thermoluminescent CaF_2:Mn dosimeters (sensitivity range of 4 mR to 5,000 R) and the two film badges were DuPont type 545 film badges (sensitivity range of 30 mR to 3 R). Since there was no venting upon detonation, the TLD's and film badges set out December 8, 1967, were left in the field throughout the GB-ER redrill period which began immediately after the shot. The GB-ER redrill was completed on January 10, 1968, and the TLD's and film badges were returned to SWRHL for evaluation.

An entirely different TLD network was established for the GB-2R redrill and GB-ER flaring operation. TLD's were stationed around the site area at 19 locations ranging from 0.1 to 6 miles from ground zero. Three TLD's were placed at each location on June 27, 1968, and collected on July 16, 1968. The locations are shown in figure 6. The station locations were selected with regard to prominent wind direction during this period of the year. During daytime hours, the wind is primarily from the southwest. A stable condition generally exists during the nighttime hours when the air 'drains' downhill toward the northwest. A network was placed 1.3 miles northwest of ground zero, covering an arc from approximately 345 degrees to 180 degrees and a network crossing the valley through which the drainage winds pass.

The results of the TLD network during periods 1 and 2 are shown graphically in figure 7. The graph shows the average exposure recorded by the TLD stations located within 30-degree arcs. Table 5 gives the results of the less extensive TLD network used for period 3. Since the locations of most of the stations and length of exposure for the two networks were not the same, a direct comparison of the results should not be made.

Table 3. Summary of Gasbuggy water analysis results[a]

Radioactivity	Number of samples counted	Number of samples above detectable limits[b]	Range (pCi/liter) Minimum	Range (pCi/liter) Maximum	Average of samples above detectable limit (pCi/liter)	Average all samples (pCi/liter)
Preshot:						
Beta	78	52	3	40	8	5
Alpha	78	25	1	23	4	1
Post-shot:						
Beta	25	14	2	29	9	5
Alpha	25	7	1	16	3	1

[a] The concentrations determined by gamma-ray spectroscopy were negligible for all water samples.
[b] Detectable limits: beta radioactivity, 2 pCi/liter based on 250-ml sample counted for 50 minutes and alpha radioactivity, 1 pCi/liter based on 250-ml sample counted for 50 minutes.

Table 4. Gasbuggy water sampling results—period 3[a]

Date taken (1968)	Sample location	Tritium[b] (pCi/liter)	Gamma spectrum analysis
June 24	Pattun ranch		Negligible
June 27	Drilling water pond, Gasbuggy road and NM 17 (2 miles north of Pond #1)[c]		Negligible
July 16	Well and Pond #1	900	Negligible
	Pond #4	700	Negligible
	Drilling water pond	<400	Negligible
	Pond (2 miles southeast of Laguna Seca)[c]	800	Negligible
	Laguna Seca	<400	Negligible

[a] Period 3: GB-2 redrill and GB-ER and GB-2R flaring operations.
[b] Alpha and beta radioactivity counts were not made on these 7 samples. The tritium concentrations given are considered to be background.
[c] Not shown in figure 5.

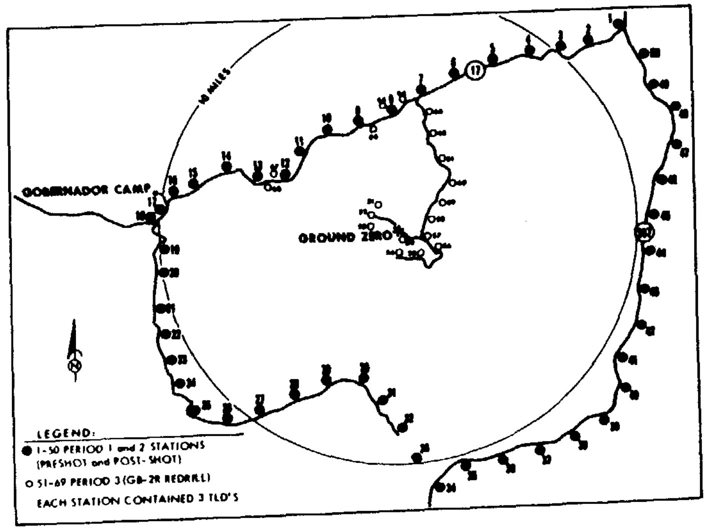

Figure 6. Project Gasbuggy thermoluminescent dosimeter stations

Table 5. Gasbuggy thermoluminescent dosimeter results—period 3[a]

Station number[b]	Average daily exposure (mR)	Station number	Average daily exposure (mR)
51	0.28	62	0.14
52	.28	63	.25
53	.33	64	.27
54	.19	65	.28
55	.28	66	.27
56	.27	67	.28
57	.34	68	.25
58	.27	69	.27
59	.25		
60	.27	Controls-Farmington[c]	.27
61	.25		

[a] The actual period of exposure was June 27, 1968, to July 16, 1968.
[b] The station locations are given in figure 6.
[c] These dosimeters remained in Farmington to provide a comparative background for the period.

Natural gas surveillance

The PHS provided surveillance of radioactivity in the natural gas produced in the area surrounding the Gasbuggy site. This program consisted for an analysis for fresh fission products and natural radionuclides of the natural gas collected preshot and post-shot from all the wells and the gathering system located within 5 miles of ground zero.

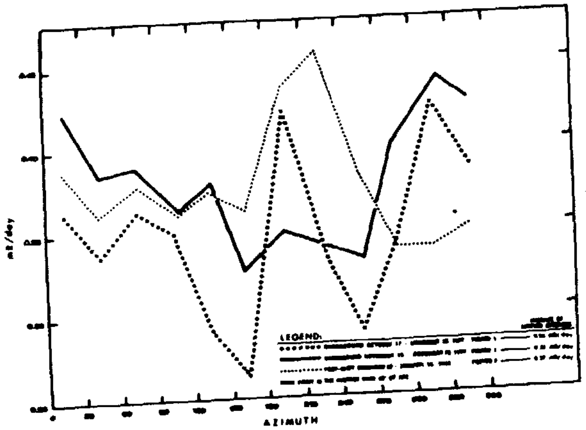

Figure 7. Project Gasbuggy thermoluminescent dosimeter station results periods 1 and 2

Fifteen samples were analyzed before the detonation to determine background radioactivity levels and 31 samples were analyzed after the detonation to check for increased levels of radiation. The results of these samples are given in table 6. In addition to the samples collected within 5 miles of ground zero, 12 samples were collected at more distant locations within the El Paso Natural Gas Company's system. The results of these samples are given in table 7. All samples were analyzed for radioisotopes of xenon and krypton, and the radionuclides, radon-222, carbon-14, and tritium. The analysis of the gas samples showed no evidence of fresh fission products in any sample. Levels of radon-222 found in post-shot samples were no higher than the background levels found in preshot samples.[4]

As of June 1, 1969, all producing gas wells within a 5-mile radius of ground zero are still physically cut from the El Paso Natural Gas Company's distribution system. Plans for sampling gas on a periodic basis from these producing wells when they are returned to the distribution system are presently being considered by the Atomic Energy Commission.

Summary and conclusions

The offsite radiological safety program conducted by the Southwestern Radiological Health Laboratory for the U.S. Atomic Energy Commission documented that no detectable radioactivity was introduced into the offsite environment as a result of the Project Gasbuggy detonation. In addition, the SWRHL was adequately prepared so that personnel could effectuate emergency procedures to insure protection of the public health had an unforeseen accident occurred.

Initial gas samples collected from the producing wells within approximately a 5-mile radius of ground zero as well as sampling of the El Paso Natural Gas Company's distribution system have showed, as of July 1968, that no migration of radionuclides into other wells has occurred.

Project Gasbuggy represented no hazard from radioactivity to the population in the vicinity of the detonation site nor has it resulted in the introduction of any device-related radioactivity into the existing natural gas distribution system of the El Paso Natural Gas Company.

[4] For additional information on radon-222 values in gas produced in the San Juan Basin, see reference 2.

Table 6. Radon analyses from Gasbuggy preshot and post-shot natural gas sampling

Well	Formation[a]	Azimuth from ground zero	Distance from ground zero (miles)	Date collected	Radon-222 concentration[b] (pCi/ft³)
Preshot samples[c]:					
San Juan 28-4 No. 26	MV	255°	5.15	9/19/67	1,068
	MV	255°	5.15	10/24/67	1,084
	MV	255°	5.15	11/14/67	1,034
29-4 No. 2	PC	274°	1.08	9/19/67	298
	PC	274°	1.08	10/16/67	(d)
30-4 No. 4[e]	PC	348°	8.54	9/19/67	(d)
	PC	348°	8.54	10/24/67	(d)
Indian E No. 1	PC	63°	1.27	9/19/67	(d)
	PC	63°	1.27	10/16/67	493
	PC	63°	1.27	11/21/67	470
Indian H No. 1[f]	PC	128°	4.70	9/19/67	179
	MV	128°	4.70	10/03/67	309
	PC	128°	4.70	10/24/67	199
	MV	128°	4.70	10/24/67	354
	PC	128°	4.70	11/07/67	204
Post-shot samples[c]:					
San Juan 27-4 No. 1	MV	179°	5.40	1/02/68	34
	MV	179°	5.40	1/30/68	9
28-4 No. 1	MV	260°	4.40	12/27/67	520
28-4 No. 2	MV	251°	4.07	12/27/67	146
28-4 No. 6	PC	218°	.59	2/27/68	288
28-4 No. 7	PC	126°	2.14	2/27/68	239
28-4 No. 8	MV	178°	4.56	1/30/68	1,028
28-4 No. 13	MV	136°	4.60	12/26/67	1,165
28-4 No. 14	MV	228°	4.50	12/27/67	230
28-4 No. 16	MV	235°	5.30	12/26/67	859
28-4 No. 17	MV	240°	3.83	12/26/67	754
28-4 No. 26	MV	255°	5.15	12/20/67	1,628
28-4 No. 27	MV	243°	5.45	12/26/67	1,080
28-4 No. 28	MV	246°	4.79	12/26/67	70
29-4 No. 1	MV	282°	5.12	12/20/67	79
29-4 No. 2	PC	274°	1.03	2/27/68	302
29-4 No. 4	PC	309°	.84	2/27/68	464
29-4 No. 10[g]	PC	344°	.10	NS	
29-4 No. 14	MV	277°	4.45	12/27/67	34
29-4 No. 16	PC	39°	.72	2/27/68	483
29-4 No. 18	MV	280°	2.49	12/26/67	592
30-4 No. 4[e]	PC	348°	8.54	12/12/67	333
Indian A No. 2	PC	43°	1.28	12/27/67	480
Indian E No. 1	PC	63°	1.27	12/27/67	52
Indian I No. 1[f]	MV	139°	5.65	12/19/67	617
	PC	139°	5.65	12/19/67	761
	PC	139°	5.65	1/30/68	9
Indian B No. 1	MV	136°	4.58	12/19/67	216
Indian L No. 2	MV	129°	3.96	1/02/68	18
Indian H No. 1[f]	PC	128°	4.70	12/19/67	244
	MV	128°	4.70	12/19/67	484
San Juan 28-4 No. 21	MV	265°	4.93	12/27/67	634

[a] Gas bearing formation: MV is Mesa Verde formation, PC is Pictured Cliffs formation.

[b] The minimum level of detection (MLD) for radon-222 is 1 pCi/ft³. Similarly MLD's for other isotopes analysed are: xenons —200 pCi/ft³
kryptons —200 pCi/ft³
carbon-14—300 pCi/ft³
tritium — 20 pCi/ft³

All radon-222 values have been corrected for decay to the time of sample collection.

[c] The table only contains concentrations for radon-222. The samples were also analyzed for radioxenons, radiokryptons, carbon-14, and tritium. Results of these analyses were all below minimum detectable levels shown in footnote [b]. All radons on preshot samples were analysed at the field laboratory in Farmington. All post-shot radon analyses were performed in Las Vegas. The Farmington analyses were carried out on raw natural gas while the Las Vegas analyses were made by separating the radon from the natural gas. The Las Vegas results have been corrected by an appropriate correction factor to make them comparable to the preshot Farmington results.

[d] These samples were not analyzed for radon-222, but were analyzed for xenon and krypton radioisotopes.

[e] This well is located outside of the area bounded by a 6-mile radius from ground surface zero.

[f] Samples from Indian H No. 1 and Indian I No. 1 wells can be taken from one or both the Pictured Cliffs (PC) and Mesa Verde (MV) formations because these wells penetrate both formations.

[g] This well is also called 10-36 and is located 435 feet from ground zero. It was stemmed prior to the event and could not be sampled post-shot.

NS, no sample taken.

Table 7. Special post-Gasbuggy natural gas samples

Sample description	Collection date (1968)	Radon-222 concentrations[a] (pCi/ft³) Average of 3 analyses
Mainline, blockvalve 3-mile post 36 +	4/24	260
24-inch main and loop blockvalve 2	4/24	404
24-inch main, blockvalve 2	4/24	475
San Juan 29–5 unit 45	4/24	713
22 MD–R.A. Mesa Verde	4/25	1,005
San Juan 28–5 unit 55 R.A. Mesa Verde	4/25	1,132
San Juan 28–5 unit 45 R.A. Dakota	4/25	275
San Juan 28–5 unit 19 R.A. Picture Cliffs	4/25	484
San Juan 27–4 unit 13 R.A. Picture Cliffs	4/25	[b] 761
San Juan 30–4 unit 4 R.A. Picture Cliffs	4/25	405
Ehrenberg meter station	4/26	164
Topok meter station	4/26	294

[a] Corrected for decay to the time of sample collection.
[b] Duplicate analysis.
Note: Each sample was analyzed for tritium, carbon-14, radioxenon, and radiokrypton. No radioactivity was detected above minimum detectable levels of 20, 300, 200, and 200 pCi/ft³, respectively.

Representative products and manufacturers are named for identification only and listing does not imply endorsement by the Public Health Service and the U.S. Department of Health, Education, and Welfare.

REFERENCES

(1) PUBLIC HEALTH SERVICE. Radiation Alert Network, December 1967. Radiol Health Data Rep 9:226–229 (April 1968).

(2) BUNCE, L. A. and F. W. SATTLER. Radon-222 in natural gas. Radiol Health Data Rep 7:441–444 (August 1966).

Dr. RADFORD. I see. Well, I think that is an omission, because clearly from the July report it is obvious that tritium has been taken up into the hydrocarbons and some discussion of the significance of this would be well worthwhile.

But I am not familiar with the purpose of the publication in front of you, so I can't react more than this.

Senator MUSKIE. Well, we would like you to look at this at your convenience. I wanted to get it in the record so that other witnesses, and especially witnesses for the agencies involved, can be in a position to respond to it at the right time.

I don't want to conclude too much from these things, and I certainly am not interested in attacking the basic integrity or motivation of the people involved in these agencies.

I relate my questions to the fact that we do become numb to the risk involved here, especially I think in these agencies whose principal mission is something other than environmental safety, even though this is a related responsibility.

So I think these questions do have to be raised. I think these agencies from time to time do need to be challenged, just as we do in the Senate from time to time, to be sure we are focusing on the public interest. That is why we raise the question.

Any further questions; Senator Gravel?

Senator GRAVEL. No.

Senator MUSKIE. Thank you very much.

(Subsequent to the hearing the following letter was sent to Dr. Radford:)

JANUARY 23, 1970.

Dr. EDWARD RADFORD,
Professor of Environmental Medicine, Johns Hopkins University,
Baltimore, Md.

DEAR DR. RADFORD: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass over any of the attached which are outside your area of specialty or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(Subsequently, the following reply was received from Dr. Edward P. Radford:)

THE JOHNS HOPKINS UNIVERSITY,
SCHOOL OF HYGIENE AND PUBLIC HEALTH,
Baltimore, Md., February 18, 1970.

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, U.S. Senate, Committee on Public Works, Washington, D.C.

DEAR SENATOR MUSKIE: In response to your letter of January 23, 1970 with the enclosed questions dealing with underground uses of nuclear energy, I have made a number of comments in the attached pages. Also enclosed is my testimony

given before the Joint Committee on Atomic Energy, January 28, 1970. This testimony supplements my remarks before your committee hearing in November.

Please let me know if I can be of further assistance.

Sincerely yours,

EDWARD P. RADFORD, M.D.,
Professor of Environmental Medicine.

STATEMENT PRESENTED TO THE JOINT COMMITTEE ON ATOMIC ENERGY

(By Edward P. Radford, M.D.)

I appreciate the opportunity to appear today as a private citizen concerned with certain aspects of the atomic energy program, and I have attached a brief biographical summary describing my qualifications. My experience with public health and radiation has included participation as a radiological health officer at the Pacific atomic bomb tests in 1948, and several years as director of the radiological health training program at the Harvard School of public Health, where I developed the first course on biological effects of ionizing radiation taught at Harvard. My teaching and research has been in several aspects of public health, including besides radiation effects, toxicology and air pollution hazards. Currently I am working on health problems related particularly to carbon monoxide in the environment. During the past two years I served as Chairman of the Sub-Task Force on Physical Factors of a Task Force assembled by the National Institute of Environmental health Sciences to evaluate research needs for the next five to ten years. Our group considered ionizing radiation, lasers, microwave and ultraviolet radiation, heat, noise, crowding and other physical problems in environmental health. Thus I am familiar with many of the environmental health problems arising from sources other than use of nuclear fuels. Some of these are also of urgent public concern, as we all know.

I have had an opportunity to appear before some of the Senate members of this committee in a hearing held on Oct. 15, 1969, before the Subcommittee on Atomic Energy of the Senate Appropriations Committee. At this time I summarized my opinions with regard to nuclear power development in a rather general way, and copies of my testimony at that time were sent to Mr. Bauser. In order to save time I would like to review briefly some points discussed in that testimony, which I will not discuss in detail today. First with respect to the economic aspects of nuclear power development, I believe that this should not be particularly relevant issue at this time. I am convinced, as are the Atomic Energy Commissioners and many other people, that we must develop nuclear power, and preferably as soon as it is practicable to do so with large scale development of breeder reactors. The question of whether power produced by nuclear plants is more or less expensive than that produced by other kinds of electric power plants, is not, in my opinion, a prime issue so far as long-range national planning is concerned. That is, the reason for developing nuclear power should not be solely because electric power will be cheaper by this means. There is very little evidence for such a statement in my opinion, and I do not consider it very important anyway, in comparison with such issues as conservation of hydrocarbons for other purposes. Much more important is that every effort must be made to protect the environment from potentially hazardous wastes from all sources, and I include those from coal-burning and other fossil-fuel plants, which currently are emitting unacceptable amounts of air pollutants. Economic costs of achieving this goal must become accepted as part of general policy.

Secondly with regard to nuclear power development, one of the central problems is the development of fast-breeder reactors. This matter has been discussed by others at length and I shall not do more than simply mention it at this time. The important environmental problem of the fast-breeder reactor program at this time is related to operational safety. Experience has not been sufficiently extensive with large liquid-metal cooled reactors that firm reassurances can be derived from their safety record, because the risk of a nuclear excursion is substantially greater for fast-breeders than for light water converter reactors. I believe that

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in time the problem of operating and controlling breeder reactors can be solved and that they can eventually assume the important role that clearly they must, as Dr. Seaborg has repeatedly stated. In other words I support the decision of the Atomic Energy Commission to press ahead vigorously with the fast-breeder reactor program, and I caution only that they not be considered commercial power units available at this time for construction near metropolitan areas, but rather be recognized only as experimental units whose safety record still must be established.

The final point of a general nature that I raised in that testimony will serve as an introduction to some problems that related particularly to the Calvert Cliffs reactor. This point is that better mechanisms must be developed for participation of public representatives in the decision-making process for the location of power plants, as well as other major industrial complexes, with particular emphasis on the ultimate environmental impact such developments will have.

I can illustrate the reason for my concern by summarizing briefly some history. The decision to build a nuclear reactor of approximately 1600 megawatts electric capacity at Calvert Cliffs was first made public by the Baltimore Gas and Electric Co. in 1967. Prior to this announcement they had actually purchased the land on which the reactor was to be built next to the Chesapeake Bay. Subsequent to that date they continued planning discussions already begun between their own engineers, the engineering company from whom they were purchasing the reactor, as well as the staff of the Atomic Energy Commission. Included of course were discussions of the safety aspects of this plant. Between 1967 and May, 1969 the Baltimore Gas and Electric Company did extensive site preparation, so called, at Calvert Cliffs. This included clearing of the area and beginning excavation for the reactor units. Thus before any public hearings were held a substantial expenditure of funds had been made by the Company. In May, 1969 public hearings of the Atomic Energy Commission required under their regulations before issuance of a construction permit, were held before a board of three reviewers in Prince Frederick, Md. The only issues which were under consideration at that hearing had to do with guarantees of safe operation of the plant, and the safety to human populations of waste discharges from it. Since the AEC had no jurisdiction over such matters as thermal pollution or effects on fish in the Bay, those aspects could not be challenged specifically at the hearing. As a witness at those hearings for one intervenor, the Chespeake Environmental Protection Association, I raised the principal concern I had, that is, the mode by which tritium would be released from this plant. On the basis of the information available then, I believed tritium might be a health hazard to a significant population ingesting seafood from the Bay. Legal counsel for the Baltimore Gas and Electric Company was quick to point out, correctly, that the challenge we made on this issue really was made against the standards by which nuclear wastes could be discharged into the environment under 10 CFR 20, the part of the Federal Code having to do with these matters.

The decision of the review board was that a construction permit should be awarded, but some members of the board indicated that on scientific grounds there appeared to be some merit in the argument we had presented. Subsequently a document was produced by the legal department of the AEC in which a statement was made that the review board had no jurisdiction over the standards presented in 10 CFR 20, and therefore our argument of the adequacy of these standards for tritium release was irrelevant. I cite this history simply to ask the question: If the adequacy of standards by which the AEC permits release of radionuclides cannot be challenged at these public hearings, and if the issues of thermal pollution, power line location, hazards from fuel reprocessing and other matters cannot be raised, what scientific basis is there for public discussion? In Bodega Bay, California, seismic problems were raised, but this was a special issue applicable to unique circumstances. I am as opposed as the AEC personnel must be to purely emotional appeals at such hearings, but if there are no really substantive scientific issues except the fact that the company will adhere to AEC standards, which clearly must be the case if plans have already been approved by the AEC staff, then it is apparent that these public hearings are pure window-dressing and serve no useful function, serving only to arouse the public emotionally.

In this case, after the construction permit was granted in June, 1969, the Baltimore Gas and Electric Company argued at a hearing of the Public Service Commission of the State of Maryland that they had actually begun construction of this power plant prior to passage, in 1968, of Maryland legislation, which put acceptance of design and site location of all power plants under the jurisdiction of the Public Service Commission. Because construction of this plant had been begun prior to the legislation, it did not apply to Calvert Cliffs, they argued. I conclude from this history that as of May, 1969, at any rate, the citizens of Maryland had no significant basis to appeal the decision made by a private utility to construct a nuclear power plant at Calvert Cliffs, or anywhere else. May I emphasize at this time that I consider the Baltimore Gas and Electric Company to be perhaps the least to blame of the participants in this controversy. They acted in good faith to meet a pressing public need, more power generation. Now, through our mistakes we learn, and I believe the states are learning that they have a vital stake in the development of power, as are federal agencies responsible for eliminating environmental pollution. To the extent that the issues raised at Cayuga Lake, in Minnesota, at Calvert Cliffs, and elsewhere have contributed to a greater awareness on the part of state and federal officials of their responsibilities in power plant development then I think we have accomplished something. With regard to the tritium issue brought out at Calvert Cliffs it is my understanding that a special committee has been constituted to investigate the standards for tritium release, and certainly I welcome this addition to our evaluation of this problem.

In the remainder of my remarks I should like to discuss the tritium issue in some detail. At the outset I want to make clear that there are some basic issues of environmental radiation standards pertinent to all radionuclides discharged from nuclear power development. My approach to these issues is somewhat different from both the Federal Radiation Council and Drs. Gofman and Tamplin. There is however, a common issue which is very basic indeed, and which I should like to mention now and discuss again with regard to tritium. That is, does the Atomic Energy Commission, its advisory staff, the Federal Radiation Council, the National Council on Radiation Protection and Measurements and any other regulatory groups, really accept the linear dose-response, non-threshold concept of several kinds of radiation effects on man, or don't they? If they do not, then they are opposing the opinions of the International Commission on Radiation Protection, and the weight of an extensive body of scientific evidence applicable to at least certain aspects of genetic and carcinogenic effects of ionizing radiation. In other words, they are not applying a scientific approach and as a scientist I would oppose them vigorously. On the other hand, if they do accept the linear dose-response non-threshold concept of certain radiation effects, then they should face squarely the political, legal, and moral implications of that fact in assessing the implications of release of radionuclides into the environment.

Standards proposed by the various regulatory agencies, including those which have been incorporated in 10 CFR 20, are expressed in terms of Maximum Permissible Concentrations, originally designed for occupational exposures but extended to general environmental releases from power plants, fuel reprocessing plants and other sources of radionuclide release. In Report No. 9 of the International Commission on Radiation Protection adopted in Sept., 1965 the idea of a "Dose Limit" has been recommended for "planned exposures of individual members of the public, and of populations . . ." This change in terminology reflects the idea that all radiation exposures to the general public should be kept as far below this value as possible. The Dose Limit is designed to restrict exposure of the gonads to less than 5 rem in 30 years, or 170 mrem per year for continuous exposure. The present values for release of radionuclides given in 10 CFR 20 derive almost completely from the lists published in ICRP publication 2, dated 1959, with Table II values for air and water one tenth of the 168 hr/wk occupational Maximum Permissible Concentration values given by the ICRP. These are estimated to give 500 mrem/year exposure to the critical organ of the body if the radionuclide is inhaled or ingested in water at the specified concentration. It is only in section 20, 106, paragraph 7(e) that the statement is made that limitation of releases to levels ⅓ this amount (i.e. equivalent to 170 mrem/year) may be required by the Atomic Energy Commission. Thus the MPC levels talked

about in many of the documents submitted in Part I of these hearings refer to concentrations in Table II and are those estimated to give 500 mrem/year dose. The cumulative dose for 30 years' reproductive life-span would thus be 15 rem from continuous exposure in air or water to any one of the isotopes listed in Table II.

For tritium the Maximum Permissible Concentration in 10 CFR 20, Table II is 3×10^{-3} microcuries per milliliter of water, or 3000 picocuries per ml. (water standard). If the factor of ⅓ is applied to take account of the possibility that continuous exposure over a period of 30 years could apply to reasonably large populations, then the figure would be 1000 picocuries per ml., equivalent to a continuous radiation exposure of 170 millirem per year, by the conventional calculation.

In 10 CFR 20 there is no discussion of standards for tritium or other radionuclides in foods. This is a problem of particular concern for effluents being discharged into a waterway having heavy production of edible seafoods, because of the fact that many radionuclides, such as those to be released from Calvert Cliffs, have the capability of becoming concentrated to a much greater extent than will exist in the water itself. Some of these concentrations can be as high as a million, but in most cases they are substantially below this value. Although we do not usually make up our diet almost exclusively from seafood, it is conceivable that small groups living near the Bay might do so. The statement that concentrations of radioactive effluents into a fertile estuary such as the Chesapeake Bay are a small fraction of the maximum permissible concentration does not necessarily insure safety for individuals who may ingest significant amounts of food taken from the Bay.

There has been much discussion on the issue of whether tritium can be concentrated in living aquatic organisms in the same way as other elements such as strontium or cobalt. In discussion of Calvert Cliffs, I never intended to make such an assertion, because in my opinion there is no evidence for it under normal circumstances. There can be an increase of tritium in relation to normal hydrogen in systems in which evaporation takes place, because the vapor pressure of tritiated water is lower than that of ordinary water. But in biological terms these situations are not of great significance, at least at this time, and I have proceeded in an evaluation of tritium releases with the assumption that the ratio of tritium to hydrogen cannot increase in the tissues of an organism ingesting tritium above that present in the food or water source. The point we were trying to make with regard to the Calvert Cliffs reactor is that tritium discharged into the Bay at the Calvert Cliffs plant could be taken up at the discharge point relatively quickly by rapidly growing organisms in the Bay. Their tissues would then remain labeled with the tritium atoms for varying times, but in many cases long after the organism had moved from the vicinity of the plant.

Thus at this time the organism such as a plankton or a crab, might have more tritium than would be predicted from the average water value after dilution into a larger area of the Bay. This process is fairly likely in the Chesapeake at the point where the Calvert Cliffs plant is to be, because it is a particularly active area of the Bay in terms of growth of organisms. Now this argument was particularly important in view of information brought out at the safety hearing in May, 1969 to the effect that the tritium released from the Calvert Cliffs reactor would be coming out in "pulses", or more concentrated amounts, periodically, approximately twice a month, and in a fairly concentrated "pulse" once a year when the reactor was shut down for refuelling. Under this regime, the organisms in the Bay could have at particular times of the year more tritium present in relation to hydrogen than the water in which they swam, not because of any true concentration effect but simply because once incorporated into certain of their molecules the tritium would remain fairly stable and not be diluted rapidly once the organism stopped growing. By not being diluted with the less radioactive hydrogen, the tritium laid down during the "pulse" would reflect the pulse concentration. I understand now that such "pulsing" will not generally be done, and thus with essentially continuous release of tritium the operation is safer.

Now let us turn to the issue of the tritium standards, and my opinion as to what I consider they should be. It is well recognized that the path in tissues of

beta radiation arising from tritium is short, with an average path length 0.9 micron (or ten thousandths of a centimeter) and with a substantial number of the beta particles having a track length considerably less than this. When genetic effects of the tritium incorporated into the components of the cell nucleus are considered it is not particularly meaningful to talk about rad doses for relatively scattered tritium decays. I shall therefore use as a unit the tritium to hydrogen ratio, that is the ratio of tritium atoms to normal hydrogen atoms present in the tissue molecules. To illustrate the use of the tritium to hydrogen ratio we may consider movement of tritium in Chesapeake Bay organisms. As the tritium is discharged from a reactor source, for example, the ratio of tritium to hydrogen will be at a certain level in the effluent water. That water may then be taken up in the tissues of many organisms of all sizes as part of metabolic processes, and thus the tritium will replace hydrogens throughout the bodies of these organisms. Some of these hydrogen pools will exchange very rapidly, as for example the body water itself, whereas other hydrogens will exchange very slowly or not at all. For example, the DNA of non-dividing cells would have most hydrogens unexchanged and thus be unlabeled under these conditions. Growing organisms will take up tritium into all parts of the tissue because the cells will be dividing rapidly.

Now, once these organisms are eaten by other organisms in the Bay, perhaps at a distance removed from the power plant where the tritium to hydrogen ratio is lower in the water because of dilution, the tritium in the body tissues of the first organisms will become incorporated into the second. In this process there can be no increase in the tritium to hydrogen ratio as the tritium moves through the food chain, only a decrease, but at the same time the tritium to hydrogen ratio can still be greater than in the water whose tritium has been diluted. One of the questions we raised at the Calvert Cliffs bearing was that the rate of this dilution process through the food chain was not adequately known, at this time. Note, however, the contrast between tritium behavior in this regard, compared with most other radionuclides, or materials in water such as DDT, which can become progressively more concentrated in the tissues of organisms as DDT is transferred along the food chain. In this regard tritium has a margin of safety when incorporated in food chains, not found with some of the other radionuclides.

I would now like to consider a special case of tritium uptake into human beings from the environment, which I will call the *equilibrium case;* that is the assumption is made that the rate of uptake of tritium from food and water is continuous and maintained over the reproductive life-span of the individual, including of course exposure of his parents prior to birth. This rather unrealistic model is nevertheless the one which follows most closely guidelines of the International Committee on Radiation Protection in establishing the Maximum Permissible Concentrations. The critical point here is that the assumption that the tritium to hydrogen ratio of all components of the body will be equal and maintained at the intake ratio indefinitely.

Under these conditions the critical exposure genetically is probably to the spermatogonia in the male testes. Because any hydrogen in the cell nucleus is equally likely to have a tritium present and to have a tritium decay, some of the tritium will be present in hydrogens in the DNA of the cells as well as in the nucleo-protein fraction and some adjacent water molecules. We may calculate the potential genetic significance of this tritium to hydrogen ratio by estimating the probability that a variety of mutations will appear, given a certain number of tritium decays in the region of the DNA over the reproductive life span of the individual. The equilibrium assumption requires that there be an equal probability at all times of the location at any site in the nucleus of the spermatogonia for the 30 year reproductive span. Now from the physical half-life of tritium we can conclude that the equivalent of 80% of all tritium atoms will decay in 30 years. In order to estimate the mutation rate from tritium decays two possibilities must be considered.

The first is that some of the mutations may arise simply from the beta particles emitted from the tritium in the vicinity of the DNA molecule. Secondly we must consider whether there can be a so-called transmutation effect, that is a likelihood that a tritium decay will in some way rearrange the structure of the DNA components as a result of the conversion of the tritium atomic nucleus into a

helium atomic nucleus with a release of a substantial amount of excitation energy. We shall consider this latter possibility first. Funk and Person, in Dec. 1969 reported in the journal, Science, that in bacteriophage, a small virus-like organism, the decay of tritium labeled in the number 5 position on cytosine, one of the acids of DNA, caused the cytosine molecule to be converted to thymine, another of the nucleic acids. This transformation occurs with a very high probability for each tritium decay. It is presumably a physicochemical process within the molecule taking place in a short period of time, and thus inaccessible to the usual cellular repair processes for a break induced in the DNA strands. Since hydrogen in this position occupies approximately 2% of the total hydrogen in the DNA molecule, then on the average 50 decays in the DNA molecules should give rise to one of these transitions, provided that the tritium labelling is uniform. The effect of a change from cytosine to thymine does not necessarily lead to a change in the coding of the DNA for specific amino acids in the synthesis of proteins, thus a true mutant will not always occur. Very refined experimental techniques are required to look for this transition, and these have not as yet been applied to human or animal sperm. It appears reasonable on radiobiological grounds however to assume that DNA of any type will show the same kind of transition with the same sort of probability as that found in bacteriophage.

In addition to this transmutation effect, there is of course the possibility of mutation arising from the beta particle emitted from hydrogens present in the DNA. The probability of both dominent lethals or point mutations arising from this source is not known precisely for human sperm, but for comparison purposes the analysis of experiments of Bateman of the production of dominant lethal mutations in mouse sperm is pertinent. Bond and Feinendigen calculated that approximately 300 tritium decays per spermatocyte and the subsequent sperm cell leads to one dominant lethal mutation. A lower number of decays may produce less extensive chromosomal damage, and this probability must be included as well as the likelihood of repair taking place. Combining the two effects, transmutation and direct irradiation from the tritium bound in the DNA, a probability of one significant mutation with approximately 50 tritium decays, over the reproductive life span of the individual seems to be a conservative estimate.

Now the critical issue from this point, is whether a lower decay frequency, even down to one tritium decay in the DNA of the spermatogonia, will lead to a viable or non-viable mutation. In my opinion this issue will have to be settled by further dose-response experiments with tritiated DNA precursors. In terms of the cytosine to thymine transition mentioned above, there is no reason to believe at this time why this should be influenced by the total number of decays, the probability remaining the same per tritium decay when labeled in that position. If we assume that 50 tritium decays (60 total tritium atoms) are required to produce a single mutation and the available hydrogen sites are 1×10^{11} for haploid DNA plus an equal amount for labelling on the nucleo protein as well as other adjacent hydrogens, we have therefore a tritium to hydrogen ratio for one mutation of 60 divided by 2×10^{11}. This gives 3×10^{-10} for the tritium to hydrogen ratio to give a single mutation, a result I consider to be on the high side if anything. This calculation has assumed that the dose-rate dependence is already included. The linear dose-response hypothesis applies to low dose rates in spermatogonia and thus we shall apply this result to lower concentrations. For example, at the MPC level, or a tritium to hydrogen ratio of approximately 10^{-12}, the additional mutation rate would be .0033 or 0.3%. This rate is approximately a 2% increase in the normal spontaneous mutation rate. Under the equilibrium assumption, that is exposure to tritium in all forms of hydrogen at the tritium to hydrogen ratio of 10^{-12}, would lead to 3300 new mutants per million births. If we take the value of ⅓ of this the number for the general population based on continuous exposure, the number of mutations would be approximately 1000 per million live births. Now I consider this latter figure wholly unsatisfactory for an environmental standard in terms of a mutation rate. The decision as to what is an acceptable mutation rate is not fundamentally a scientific decision. It is a collective, almost philosophical, decision, and brings up another point which I would now like to discuss.

If we are to proceed with nuclear power development, and accept that a linear dose-response curve is highly probable for at least some of the genetic and

carcinogenic effects, we must accept some risk from environmental radioactivity. We can translate that risk into the number of new mutations per generations per exposure from whatever source. We must also recognize that radiation exposures will not come from one single radionuclide present in the environment, as is now abundantly clear from the rapid proliferation of nuclear reactors with their attendant fuel reprocessing, as well as the potential use of atomic energy for underground detonations. In addition there are radiation sources such as color television sets, transient exposure to increased radiation in high altitude flights, as well as the exposure of the general public to the genetic radiation doses from medical x-rays. In the United States where medical services are extensive, exposure from medical x-rays is very widespread and will continue. Estimates are that on the average each person received about 55 mrems/year from medical x-rays, but obviously this is zero for many people and substantially more for others. We also must recognize that there are other agents in the environment which have mutagenic qualities, including many chemicals and possibly even microwave radiations or radar. If we collectively decide therefore that an additional mutation rate of a certain percent is an acceptable risk in our technologically evolving society, then it is not reasonable to assign all this genetic risk to any one component in the environment. Bear in mind that decisions made now can influence generations to come for centuries. Once we decide what an acceptable genetic risk may be, as for example one per 10,000 or a so-called 10^{-4} risk, as defined by the ICRP publication 8 then we must apportion this total to the various possible sources on the basis of their likelihood of coming in contact with large populations. I believe that new mutagenic chemicals and other environmental factors should be "assigned" half of this total mutation increase, chiefly because their potential hazard is unevaluated at this time.

The remainder if for radiation exposures, and we must assign possible sources their "share". Most important of these latter obviously is medical x-rays and we might assign 20% of the genetic risk to this rather universal source of exposure. As methods improve this might be reduced. Finally of the remaining 30% of the total we need to apportion these to radionuclide exposure in the environment depending on how likely different isotopes are to reach large populations. One of the major components of environmental radiation exposure from nuclear energy use will be tritium, particularly from releases from nuclear fuel reprocessing plants. The reason for this is that tritium is difficult to contain wherever it is present as water. The other fractions of gonadal exposure might be assigned to external sources such as krypton 85 and the third portion collectively to strontium 90, cesium 137 or all other gonadally significant internal emitters, control of which is easier than for tritium and krypton. By this assignment of the allowable genetic increment to multiple sources we therefore can assure that genetic risks will not be added without bound.

The standards thus derived for tritium can be considered emission standards, that is as long as all environmental emissions fall below this standard value then the tritium to hydrogen ratio cannot exceed safe limits. (This approach will relieve a lot of the necessity for environmental monitoring). For example, if the intake pipe of a light water reactor is bringing in water with 5 units of tritium and the standard for emission is 10, then only 5 can be added by that plant. In this way the question of crowding of multiple effluent sources in single regions takes care of itself as far as tritium releases are concerned. If environmental levels rise, it will become difficult to meet the emission standards and therefore it will be advisable to locate new sources away from the others.

I consider that a total genetic risk of 10^{-4} (one in ten thousand) is acceptable and on this basis I would assign the fraction to tritium of one tenth or 10^{-5}. From the calculations given already this mutation rate would result from a tritium to hydrogen ration of $3x10^{-18}$ uniformly and continuously present in food, particularly. If the tritium is present in water only, the likelihood of equilibrium labeling of the spermatogonial DNA is less, and thus the standard could be higher for drinking water.

I have stated that the assumption of a uniform concentration of tritium continuously present, in food and water is unrealistic, thus we should consider nonuniform exposures, say from a brief exposure for a month to food containing tritium well above background. This exposure would lead to a difference between the tritium to hydrogen ratio at the time of uptake into the spermatogonia and the effective ratio in the cells, the reason being that these cells continuously divide

and thus are making new DNA for new sperm cells. Unless the tritium to hydrogen ratio continues to be kept at the higher levels, the tritium atoms will become progressively diluted by new hydrogen atoms brought in during synthesis. Thus the risk will be less, since only the tritium atoms that decay will produce any effects, and in a few weeks they will have little probability of decay.

A different situation exists in the female germ cells, however. The ova in the female are developed when the female child is in utero, and by the fifth month of pregnancy the developing girl has the maximum number of ova she will ever have. At birth the number of ova has greatly declined by resorption of most of those developed. Subsequently the ova remain in a quiescent state until after puberty when usually one ovum reaches maturity each month and is ovulated. During the years prior to ovulation no DNA synthesis occurs and thus a substantial part of the DNA hydrogens remain fixed. If there are tritium atoms substituted for these hydrogens they will have ample time to decay.

We may therefore make calculations for the probability that mutations will occur as a result of tritium being incorporated in DNA of developing female ova during a relatively short period of exposure of the mother to tritiated food. The procedure is similar to that for the spermatogonia but with some differences. First the tritium labeling is not likely to be uniform within between oocytes or within single ones, but this factor is probably not important. Second, DNA repair processes in early oocytes, at least, appear to be more efficient than for spermatogonia, thus the effects of individual random tritium decays may be more readily repaired. On the other hand, human ova evidently become more susceptible to mutations after many years of quiesence, particularly with respect to division errors such as arises with mongolism. Whether such effects will be modified by tritium decays in the nucleus is not known. Finally the extent to which non-exchangeable hydrogens are present in the adjacent nucleo-protein of the chromosomes is not known. Again, as far as the transmutation probability is concerned, this should be the same per haploid DNA as for spermatogonia.

There are more uncertainties in calculating the tritium decays required to produce one mutation per female germ cell than for the male, but it appears that the number is not likely to be greater by more than a factor of 2. Until experiments have been done, it seems prudent, based on the results of neutron irradiation of females, to assume that the tritium to hydrogen ratio laid down in the DNA of developing ova and capable of producing one mutation in that ovum is also about 3×10^{-10}. There are no experiments available at this time to permit more than an estimate of this factor.

Because exposure of tritium in food are likely to be irregular and variable in amount, the food standards ultimately may be based on reasonable estimates of ingestion of tritiated food by women in the first half of pregnancy. It is urgent to obtain an experimental basis for such standards.

CONCLUSIONS

1. I believe that we must develop nuclear power, but the scientific evidence indicates that mankind must be prepared to accept certain risks from release of radioactivity into our environment as a result of use of nuclear energy. Some of this radioactivity has the capability to produce genetic mutations in man, and this is an important risk we must evaluate.

2. Tritium is probably the most important radioactive waste from nuclear energy use, chiefly because it is difficult to contain at nuclear fuel reprocessing plants. Presently at West River, N.Y. the small creek running from the plant has an average of 600 picocuries per ml. with occasional values over pc/ml. Tritium is also produced in sizeable amounts in pressurized water reactors, but these can apparently be operated to avoid significant concentrations of tritium release.

3. Evaluation of the probability that continuous ingestion in food and water will produce mutations in males indicates that a tritium to hydrogen ration of 3×10^{-10} will give one mutation per male sperm. (This tritium level is probably close to the sterilizing dose if uniform tritium label is probably close to the sterilizing does if uniform tritium labeling of all hydrogens really were present.) On the assumption of a straight line relationship between total decays and mutation probability the present MPC levels if continuously ingested would lead to 3300 new mutations per million births, or about 12,000 new mutations per year in the United States.

4. A better approach to establishment of standards for genetically important agents in the environment is to establish an acceptable *total genetic risk*, and then apportion this risk among those environmental agents, such as ionizing radiation, likely to increase mutations. A tentative apportionment of an acceptable risk of 1 per 10,000 (an increase of spontaneous mutations of about 0.1%)

	Percent
Chemicals, microwaves, other mutagens besides ionizing radiation	50
Medical X-rays	20
External radiation sources (krypton)	10
Tritium	10
Other genetically significant internal emitters	10

5. For intermittent tritium exposures women pregnant with female children are particularly vulnerable during the first half of pregnancy, when oogenesis is occurring. It is difficult to estimate mutation rates from tritium incorporated in the ovaries of these fetuses, but it will probably be of the same order as for spermatogonia continuously exposed to a fixed tritium to hydrogen ratio. Further experimentation is needed to settle many unsolved questions, but for the present a conservative position is warranted.

6. The problem of environmental protection in the developing nuclear era is just one aspect of the general issue of environmental pollution. The controversy concerning nuclear power development will be salutary in the long run if it awakens the public, as well as local, state, and federal officials to their responsibilities for tackling the whole range of environmental problems we face today.

BIOGRAPHICAL SUMMARY

(By Edward P. Radford, M.D.)

Education.—After graduation from Phillips Exeter Academy in 1940, I attended Massachusetts Institute of Technology 1940–1943 (no degree), and graduated from Harvard Medical School, 1946. During military service in the U.S. Air Force, 1947–49, I attended Radiological Safety School, Edgewood Arsenal, Md. and served as a radiological safety officer, Kwajalein Atoll, during the Operation Sandstone atomic bomb test series.

Professional experience.—From 1949–1952 I was Teaching Fellow and Instructor in the Department of Physiology, Harvard Medical School. From 1952 to 1965 I was Associate and later Associate Professor of Physiology on the faculty of the Harvard School of Public Health, except for a period from 1955 to 1959 when I was a staff physiologist at the Haskell Laboratory for Toxicology and Industrial Medicine at E. I. du Pont de Nemours and Co., Newark, Del. At the Harvard School of Public Health I was responsible for the Radiological Health training program. In 1965 I became Professor and Director of the Department of Environmental Health at the University of Cincinnati College of Medicine. In 1968 I came to my present position at The Johns Hopkins.

Major research interests.—My major research interests include pulmonary and renal physiology, radiation carcinogenesis, movement of isotopes in the biosphere, and environmental health, particularly air pollution effects on man.

(The questions and answers appear in appendix IV.)

Senator MUSKIE. Our last witness of the morning is Dr. Cyril L. Comar.

All right, Doctor.

STATEMENT OF DR. CYRIL L. COMAR, CORNELL UNIVERSITY

Dr. COMAR. Thank you, Senator Muskie. The privilege of appearing before this subcommittee is very much appreciated. May I acknowledge the help and presence of Dr. John Thompson of our staff on my left?

In view of the tenor of the discussion so far, I find myself in the nonsilent minority. I have no trepidation in the present setting.

I would like to clearly state at the outset that I personally have no reason to promote or be for atomic energy or nuclear technology, just as many of those who have preceded me are not against nuclear technology.

I am not callous or indifferent to human suffering, whether it is 16,000 cases a year or 16 cases a year. Although we have to compare risk from radiation with other risks of civilization, as you yourself have mentioned, we want to be sure we don't use this as a justification for any additional risk that we would impose unnecessarily from radiation.

Now, I think it is very important to comment on Dr. Gofman's testimony of yesterday and I would like to do that before I get into my regular statement.

Senator MUSKIE. Yes, indeed.

Dr. COMAR. There are three points that need to be made. The first one is whether serious consideration at official levels is given to such proposals as Dr. Gofman has made. Now I can't speak for the Federal Radiation Council, but I do happen to be chairman of the National Academy of Sciences Committee that is advisory to FRC, and I will certainly place this matter on the agenda of the next meeting of this committee.

I can't say what our committee will do with it but if they don't take it under consideration they will certainly have good reasons for that, so there is no question but that such proposals do get serious consideration.

Second, I think a brief comment should be made on the substance of the matters that were raised. I don't think this is a place to argue the technical interpretations pro and con because this really should be done around a table in the committee with the various disciplines well represented.

Because Dr. Gofman made such an apparently persuasive case, I think that it is only fair that in the record there should be a statement that there are many, many responsible scientists who disagree, at least up to this very time, with many of the assumptions that were made and with many of the numbers and the meanings of the numbers that were calculated.

I think it should be noted that the FRC values are in accord with the recommendations of the most knowledgeable scientists, both internationally and nationally. In other words, there are groups that operate as the International Committee on Radiological Protection, the National Council on Radiation Protection, the United Nations Scientific Committee on the Effects of Atomic Radiation.

These people as individuals and groups have no preconceived interest. They have no mission or charter to promote atomic energy, they have no commercial interests. They are simply trying to get at the facts and it is on the basis of these types of findings, which has already been pointed out are reasonably up to date, that the various types of standards are established.

Now, finally as a third point, we must assume that nuclear technology will produce, some finite amount of harm to the population regardless of what it is. Surely, the issue is not the absolute biological

cost to society, but it is the net cost to society, the benefit as opposed to the risk.

We have heard a lot about this and I will talk about it a little more, later but we have to do more than give lipservice to it. This is a real issue and we really have to study what are the real benefits and risks.

I think everyone is aware that over the past 50 to 60 years we have instituted many technical advances, each of which has a biological cost. For example, penicillin (people are killed from allergies that develop from penicillin), automobiles, as you mentioned, electricity, bridge building, and so forth: but if you look at the quality of our life today as compared to 50 years ago, the lifespan, and so forth, it has definitely improved so that the net effect has been a plus.

Now, what I am saying is that we will have to control our technology as we move along but we can't do it on the basis of just looking at the biological cost by itself and trying to consider that in and of itself.

We have to deal with the whole picture.

I should like now to proceed with my formal statement.

I. INTRODUCTION

I do believe that our technological priorities have to be ordered and controlled for the public good, and this has to be done by very close cooperation of Government, scientists, industrial management, and members of the public. To that end, the present bill represents an appropriate step; but accomplishment is bound to be most difficult because of the great scope, vested interests, and present lack of qualified personnel in specialty subject areas.

I should first like to emphasize some general points of view. Many of them have been expressed elsewhere and may be voiced in these hearings by others—but I feel compelled to state them again.

This is because I sense that countrywide, we may be setting the stage for seins of inaction as well as of action especially in regard to nuclear technology. The problems now facing us are similar in many respects to those posed by fallout from weapon tests.

Because of the vast amount of data available and my own personal interest in fallout, I propose to present the principles that have emerged as a starting point for assessment of present and future circumstances.

Concern about the implications of environmental radiocontamination was generated when the testing of nuclear weapons began. Since that time, millions and millions of dollars and the intensive efforts of large numbers of the best scientists all over the world have been devoted to study of the various aspects of fallout and radiation effects.

This preoccupation with fallout was justified on the basis that radiation exposures to the population from peacetime applications were insignificant as compared to those from the testing of nuclear weapons.

But times have changed and the situation may be entirely reversed. Hopefully, problems of radioactive contamination from military applications will become nonexistent; on the other hand there are peacetime applications of nuclear energy that appear potentially so advantageous to society that they could become widespread on a scale not heretofore taken into account.

To deal with this matter, we need to utilize our broad experiences benefitting from past mistakes while—and there certainly have been many past mistakes—at the same time avoiding conceptual constraints in facing the new types of problems that may arise.

In recent years there has been a very important development that must and should be taken into account—a development that has definite positive aspects but which if misguided, could be a disservice to the public good. I refer to the growth of public involvement, public reaction.

II. GROWTH OF PUBLIC REACTION

Public reaction during periods of weapons testing was generally muted presumably because: (a) decisions were made by governments in the national interest, (b) all were equally affected and there appeared to be little personal involvement and (c) there were no alternative options. It was relatively easy to convince persons that the real danger to mankind was nuclear warfare itself and not the fallout from weapons testing. And we ought to think of that point in consideraiton of why the Test Ban Treaty came into effect.

Today, of course, there is an upswelling of public opinion particularly in regard to developments in nuclear power production and underground activities. The reasons are readily understood. There is an increasing sensitivity towards the need for maintenance of the quality of the environment—we have all seen the results of our traditional approach which is to use the environment maximally for disposal of wastes, and when the damaging effect has been proven beyond all doubt, then to attempt remedial action.

Decisions on locations of nuclear powerplants are made often by private companies with economic operation usually the primary factor. Local communities and individuals feel that they should be involved in decisions because they may be personally and uniquely exposed to the contamination.

People tend to argue emotionally about potential hazards from radioactive effluents whereas their real concerns may be overpopulation and the tendency to desecrate the countryside with power reactors and industrial plants in order to maintain our standard of living.

It is especially important that persons other than nuclear scientists and industrialists contribute to decisions that affect ecological and socioeconomic conditions. Public discussion and decision should be encouraged. If it is reasoned and causes those with responsibility to be more critical, all to the good. But if public discussion and decision is emotional, caters to the sensational for the sake of sensationalism, and disregards accepted knowledge, then the net effect could well be a cost to society. For with a burgeoning population, we shall need all the net technological benefit we can get to maintain our standard of living and quality of life.

Perhaps the greatest manifestation of the one-sided approach is the tendency to think about, work on, and publicize the biological cost of

a given technical endeavor without regard to the benefit, or to put it another way—to give no thought to the biological cost of not undertaking a given technical endeavor.

Thus, we must seriously attempt to assess risk versus benefit. How many proposals and articles have we seen in which the author pays lip-service to "risk versus benefit" in the opening paragraph and then proceeds to the horror story of risk with never more a thought to the benefit?

III. RISK VERSUS BENEFIT

The important decisions must be made either consciously or intuitively on a balancing of risk versus benefit. This is often a most difficult task. Not only must we know, what is the cost? what is the benefit? but who pays the cost? who gets the benefit? In many instances not only must data be obtained for the technical operation in question, but comparisons must be made with the biological costs of alternate operations.

Senator MUSKIE. Dr. Comar, hasn't the reverse been true? Have we not tended to push forward technologically because of the benefits without adequate consideration of the risks?

Dr. COMAR. Yes, I am sure we have, and each instance has to be looked upon on its own merits and I think it would be arguable, for example, whether human society has undergone a detriment overall from the use of the DDT up to now as opposed to the case if it had never been put into effect.

I think these matters should be discussed and my whole point is that we don't want to go ahead where we can foresee dangers, but on the other hand, we don't want to deprive ourselves by being unrealistic about the possible effects.

I think there has to be a balance. I would certainly agree with you on that.

Senator MUSKIE. Also, isn't it the reluctance to discuss the risks by those who are in a position to enlighten the public that results in sensationalism? I think the greatest fodder of sensationalism is often the absence of good information.

Dr. COMAR. I haven't felt that there was any great reluctance to discuss the issues as they arose and the type of sensationalism that I deplore is the recent article in Esquire, for example, the full-page ad in the New York Times. This was in reference to Dr. Sternglass' paper.

Senator MUSKIE. Well, just the other day I flew up to my own State from Boston. It was a beautiful bluebird day, as we describe it in my part of the country, and as I flew over Boston—which is about as exposed to the cleansing effect of ocean breezes as any city on the continent, I saw the worse case of air pollution over a city that I have ever seen.

There isn't anything worse that I have ever seen in Los Angeles and New York, two cities closely identified in the public mind with smog. It was a black smog and in stark contrast with the beauty of the day.

Now, when the public sees this sort of smog and the evident failure of industries in their area to take advantage of existing technology which

has resulted in this smog, isn't that a temptation to resort to sensationalism?

Dr. COMAR. Well, I think it is, and we want to make sure that it is not misguided, but don't misunderstand me. I agree 100 percent that we should have more and more control of priorities over our technology and of our industry and this is why I like to live in the little town of Ithica rather than in other towns where there is smog and one breathes in the polluted air.

But I think what I am really saying is that one has to start now looking at the relative risks.

Senator MUSKIE. I agree with this, but my point is that the emphasis for too long has been in the opposite direction, that is, in concentrating on the benefits of technology, and not the risks. Because it has been that way for so long the old pendulum of the clock has swung and there is now a tendency to indulge in sensationalism to express the concern which the public feels, and I am not sure that the pendulum is going to stop just because we don't like the sensational aspects.

The sensationalism will continue until the public gets the kind of results that it wants. I am fully in accord with the need for balanced judgment, but I suspect that, historically, in order to achieve a balance you are going to have to put up with a little more sensationalism for awhile.

Dr. COMAR. Yes, well, I think and I hope you agree with me that we should try to see that efforts are directed toward the problems that are most important. Let me give you one example: We have talked a lot about the population guides for radiation.

Estimates have been made that if everyone were exposed to the present population guides the risk would be about equivalent to that which a person undergoes if he drove to and from work each day when he lived about 6 miles from work.

This is the order of the risk. Now if we devote our efforts to trying to minimize that type of risk and are distracted from some of the real problems of the day, air pollution and so on, then I think we are essentially going to be wasting a good deal of our life.

So my whole point is that one has to make a reasoned, logical approach to these problems and to do the best we can to increase the quality of our levels.

Senator MUSKIE. Well, reason doesn't do the whole job. It is the values that are considered important to which you apply the process of reason. Obviously, the witnesses yesterday had a somewhat different sense of values than perhaps you and I do, so their reason leads them to a different conclusion about the risks than yours.

Senator Gravel?

Senator GRAVEL. Speaking of the emotionalism, I can give you an example where probably a franker, more candid discussion might have avoided the last flareup in Alaska. Many people other than Alaskans were involved. The Sternglass ad made no reference to Amchitka test at all, but these people used these tactics because they couldn't get a legitimate hearing.

It was not until the 11th hour that the hearing was granted. You

couldn't even get copies of legitimate, pertinent reports by some sci-
entists. Accounts like this, let us say overreacting on the part of the
Nation, on the part of individuals, indicate that there is some problem
and the best way to solve that problem, of course, is through an in-
terchange of ideas and information.

I share your view, Senator. The emphasis has been on the other side
very strongly.

Senator MUSKIE. The question is which is more irresponsible, over-
emphasis of the risk or underemphasis of the risk?

Dr. COMAR. Since we are all reasonable men, we walk the middle
line, you know.

Senator MUSKIE. It is not easy to find. Is it .17 or .017?

Dr. COMAR. Well, I think you would agree with me at any rate——

Senator GRAVEL. Excuse me. I had one more point.

Dr. COMAR. Sorry.

Senator GRAVEL. Because of air pollution from the automobile, soci-
ety has such a large economic investment it is difficult to correct it at
this particular time. What many of us are trying to do is make the
corrections before the tremendous investment is made in this new tech-
nology which will at a point in time make it difficult to correct.

It would seem cheaper and wiser to correct it as we go along and
that is probably the reason for emphasis at the beginning rather than
when the problem develops.

Senator MUSKIE. Doctor, I have to leave for a few minutes. I might
not return before you complete your testimony. Senator Gravel will
take over and I hope I will be back while you are still testifying.

Senator GRAVEL (presiding). Please continue, sir.

Dr. COMAR. I did want to make further points about risk versus
benefit because again it seems to me that this is really the substantive
issue and if the benefit is minimal, then the decision comes easy. To
resume where I left off in my statement, I have cited the case of the
glutamates.

For example, glutamates added to baby food make it taste better
to the mother and serve no other purpose, thus the benefit is nil. There-
fore, if there is the flimsiest evidence or the slightest indication that
baby foods with glutamate may cause harm, the decision is an easy
one, off the shelves they come.

In contrast, the situation in regard to nuclear power production
is much more difficult. We will need certain amounts of power in the
future: how do we decide which will have a lower biological cost, fossil
fuel plants or power reactors at some given degree of waste con-
tainment?

Too often, the public proclamations, and more important the re-
search studies themselves completely neglect the benefit comparison.

Mr. Chairman, the remainder of my formal statement contains much
technical detail and citations of reference material. With your per-
mission, I would like to forego further reading and submit the unread
portion for the record.

Senator MUSKIE. Without objection, it is so ordered.

(The remainder of Dr. Comar's prepared statement follows:)

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IV. ENVIRONMENTAL RADIOCONTAMINATION

To turn to more technical matters, one can regard environmental radio-
contamination in the engineer's operational terms as a simple "black box" system.

radioactivity → Biosphere → harm to society

The input is the radioactivity released to the environment; the output is harm to
society. What we require to know is the relationship between input and output.
But fortunately, direct observations of harm cannot be made because the levels have
not been high enough to produce detectable effects. So we must break down the
system and study the individual steps in order: a) to try to get an indirect approach
to the desired relationship, b) to gain an understanding of what is happening and
c) to be able to plan modifications or counter-measures to be taken if ever necessary.

The actual system is tremendously complex. In essence the input radioactivity
may move through all parts of the environment by varying pathways and we must try
to estimate the time pattern of levels of radioactivity in all parts of the environ-
ment including living organisms, the food chain, and man himself. The next step is
to translate these levels into radiation dosage delivered, and finally to convert
radiation dosage into expected harm. It should be obvious that there are un-
certainties that multiply in going from one step to the next. For fallout there
are good data on the relationships between radioactivity released and amounts that
end up in the biological organisms; the main uncertainty is in the harm from low
levels. For peacetime uses, we still need much more experience with distribution
behavior.

radioactivity → Levels in all parts of environment → Radiation dosage in biosphere → Harm in biosphere → harm to society

This very complexity led to an important guiding principle for dealing with
fallout studies. I refer to the concept of "Critical Factors." The term critical
is used to denote those factors which given rise to the greatest risk and do so to
such a degree that other factors make no significant contribution to the risk.
Emphasis on the critical factors allows us to avoid wasted effort, to focus on
important matters that require immediate attention, and to reduce the problems
to manageable dimensions. Of course, we must always be alert to seek out any factors
whose criticality may have been overlooked or that may become critical because of
changing conditions. The general categories of critical factors include: a) the
organisms, b) the radionuclides or other agents, c) the pathway through the biosphere,
d) the specific population, e) the specific tissue or organ.

A. The critical organism: In regard to fallout contamination, man has been
considered the only organism whose welfare must be guarded. It is difficult to
envision a circumstance in which radiation effects on lower organisms could be
observed without previous or simultaneous exposure of man at unacceptable levels.
In regard to radiation from peacetime applications, I would hold to this concept.
However, for other types of effects such as caused by thermal releases or earth
moving operations, there needs to be serious consideration of direct effects on
ecological systems.

B. The critical radionuclides: The critical radionuclides of fallout are conceded to be ^{131}I, ^{90}Sr, ^{137}Cs, and ^{14}C. The latest values for the dose commitments for populations in the north temperate zone from nuclear tests carried out before 1968 are given in Table 1 taken from the recent report of the United Nations Scientific Committee on the Effects of Atomic Radiation (1).

Table 1

Dose Commitment in North Temperate Zone from Nuclear Tests Carried out Before 1968(1).

		Dose in mrad
Gonads		
External	Short-lived	36
	^{137}Cs	36
Internal	^{137}Cs	21
	^{14}C	13
		110
Cells lining bone surfaces		
External	Short-lived	36
	^{137}Cs	36
Internal	^{90}Sr	130
	^{137}Cs	21
	^{14}C	16
	^{89}Sr	<1
		240
Bone marrow		
External	Short-lived	36
	^{137}Cs	36
Internal	^{90}Sr	64
	^{137}Cs	21
	^{14}C	13
	^{89}Sr	<1
		170

The values in Table 1 represent the radiation dose to the population from nuclear tests conducted before 1968 expressed as the cumulative exposure expected to be received by the year 2000. Several points can be made: a) these values are based on the collection of large amounts of data and highly refined interpretations of analytical nature, b) for comparison it should be noted that the dose from natural background radiation is about 120 mrad for a single year and about 5000 mrad for a comparative period of time, c) except for ^{131}I, none of the other radionuclides dispersed in the fallout produced radiation doses anywhere near those indicated in Table 1. Higher doses, particularly to the thyroid gland were sustained during times when tests were in progress; these dose commitments can only be estimated for local groups.

As an aside these data allow us to answer retrospectively and with confidence some of the burning questions of those times - Should the dangers of fallout limit the testing of nuclear devices? No, in view of the benefits gained and the low cost. Should we worry about the dangers of fallout? Governments should, but individuals should have no more anxiety than when they drive their cars.

Unlike the testing of nuclear weapons in the atmosphere, all of the steps in power reactors and associated reprocessing plants can be kept under control and thereby the releases of radiocontaminants can be regulated. Thus although reactors accumulate large inventories of fission products within themselves, the amounts released can be kept relatively low. The same fission products as in fallout need to be taken into account (^{131}I, ^{90}Sr, ^{89}Sr, ^{137}Cs). Depending upon construction materials some induced radioactivity in corrosion products may be released (^{60}Co, ^{59}Fe, ^{51}Cr, ^{54}Mn, ^{55}Fe). Releases to the atmoshere include ^{85}Kr, tritium, and ^{131}I. Because krypton is a noble gas, it is unreactive and difficult to remove from the gaseous release. For the same reason, it is biologically unreactive; that is, it does not become incorporated into metabolic products or retained in an important way in tissues. This greatly simplified estimation of biological effects from it. In contrast, tritium does get built into the biologically important molecules of the body and its possible effects require careful consideration. The effects of ^{131}I are considerably reduced by its relatively short half-life. In addition, this radionuclide has been so widely studied that there is reasonable confidence in monitoring and assessment of potential harm from it.

In nuclear devices used for excavation projects there are five categories of radioactive products (2):

(1) Fission products (e.g. ^{131}I, ^{90}Sr, ^{137}Cs).
(2) Residual fissionable or fusionable materials (e.g. ^{239}Pu).
(3) Thermonuclear-reaction products such as tritium and ^{7}Be.
(4) Induced activities from device materials (e.g. ^{54}Mn, ^{56}Mn, ^{55}Fe, ^{185}W).
(5) Induced activities from surrounding materials such as rock (e.g. ^{24}Na, ^{32}P, ^{45}Ca, ^{54}Mn, ^{55}Fe, ^{59}Fe).

A considerable amount of theoretical work has been done to assess the relative importance of the large number of radionculides that would be produced in construction of the Atlantic-Pacific Interoceanic Canal. In general, the explosives have very small fission-to-fusion ratios which produce small amounts of fission products. Efforts are presently being made to reduce the fission yield even further; also internal components will be changed to decrease production of certain induced nuclides. James and Fleming (3) arranged the radionuclides in decreasing order of significance assuming an exposure of 50 years starting two months after the last detonation; their relative ranking was as follows: Tritium, ^{90}Sr, ^{210}Pb, ^{137}Cs, ^{106}Ru, ^{185}W, ^{181}W, ^{144}Ce, ^{54}Mn. All other radionculides were less significant than ^{90}Sr by a factor of more than a hundred. Fleming (4) has recently prepared another ranking for radionuclides produced in a single cratering detonation using the latest explosive design. A typical ranking in decreasing order of significance assuming an exposure of 30 years starting one week after detonation was as follows: ^{185}W, Tritium, ^{181}W, ^{90}Sr, ^{131}I, ^{210}Pb, ^{137}Cs, ^{203}Hg, ^{202}Tl, ^{106}Ru. Another approach has been the so-called specific activity method (5). In my opinion, experimental data are badly needed, to determine whether the actual biological behavior is reasonably reflected by such calculations.

C. Critical Pathways: Radioactivity from nuclear detonations falls on land and water alike. Because most of man's food is produced on land, the terrestrial pathways are of major importance. The specific pathway depended on the physical and metabolic characteristics of the radionuclide and the critical pathways are indicated schematically as follows:

^{131}I air → plant surfaces → milk → thyroid
^{90}Sr air ↗ soil
↓
↘ plants → animal diet → milk → bone
^{137}Cs air → plant surfaces → animal diet → milk and meat → muscle

The pathways of importance relative to reactor outputs are governed largely by the specific location and type of reactor but generally the acquatic route becomes more important than the terrestrial. For excavation processes the important pathways are also dependent on the location and nature of the detonation. An illustrative example of the generalized transport system for the proposed sea-level canal is given in Figure 1 (6). Obviously, a meaningful handling of data would require considerable simplification of the basic model.

D. The specific population: It is always important to be sure that no specific population group or individual is going to be exposed to unexpected levels of radiation by virtue of peculiar circumstances. There have been several examples: individuals in the subartic regions have shown doses of ^{137}Cs some 100 times higher than the average for the northern hemisphere because of special ecological conditions (7,8,9). In the United Kingdom, the output of the Windscale reactor was literally governed by a critical group who consumed large amounts of a local bread made from seaweed that accumulated ^{106}Ru; later on, the chemical processing changed so that there was an individual who became critical because of his habit of spending several hours a day on the nearby mud flats (10, 11).

E. Critical Tissue: The critical tissue or organ is determined primarily by the metabolism of the critical element. The primary radiation effects in addition to response from total body radiation are related to: a) genetic effects resulting from exposure of the gonads, b) bone cancer from exposure of bone surfaces, c) leukemia from exposure of bone marrow and d) thyroid carcinoma. Radionuclides such as ^{137}Cs, ^{14}C, and tritium can expose the whole body; radionuclides of strontium expose primarily bone and bone marrow; ^{131}I irradiates primarily the thyroid gland. Because of the obvious criticality of ^{131}I, ^{90}Sr, and ^{137}Cs much work was done on them and we know a great deal about their radiobiology. We need more information about the role of tritium in biochemically important compounds with low turnover, about the significance of radionuclides such as ^{55}Fe that become incorporated into storage proteins, the relative importance of liver accumulation of plutonium as compared to lung deposition. In addition, there is a question of synergistic action whereby co-factors could increase the harm from given levels of radiation. An outstanding example is the contribution of smoking to the increased incidence of lung cancer in uranium miners exposed to radon daughters.

V. Remedial Measures

It seemed only reasonable and prudent that thought be given to development of remedial or counter measures to mitigate if needed, the effects of weapons testing and of nuclear warfare. The general principles and recommendations have been discussed in detail (12, 13,14). A most important point is that the health risk of the remedial action must be less than the risk from the contamination at the action level. In other words, the remedial measure must do more good than harm. At low levels of contamination in accordance with official guidelines, the radiation risks are so small that almost any remedial action would have greater risks. Thus action levels for remedial action are usually much higher than those recommended for normal operating conditions.

The common way of looking at the matter is to consider two types of situations.

A. Controllable Operating Conditions. These are planned and executed so that certain agreed dose limits are not exceeded.

B. Uncontrolled Situations. These can only be remedied by action against the environmental contamination rather than against the source. As indicated above, since dose limits recommended for "controllable" conditions are quite

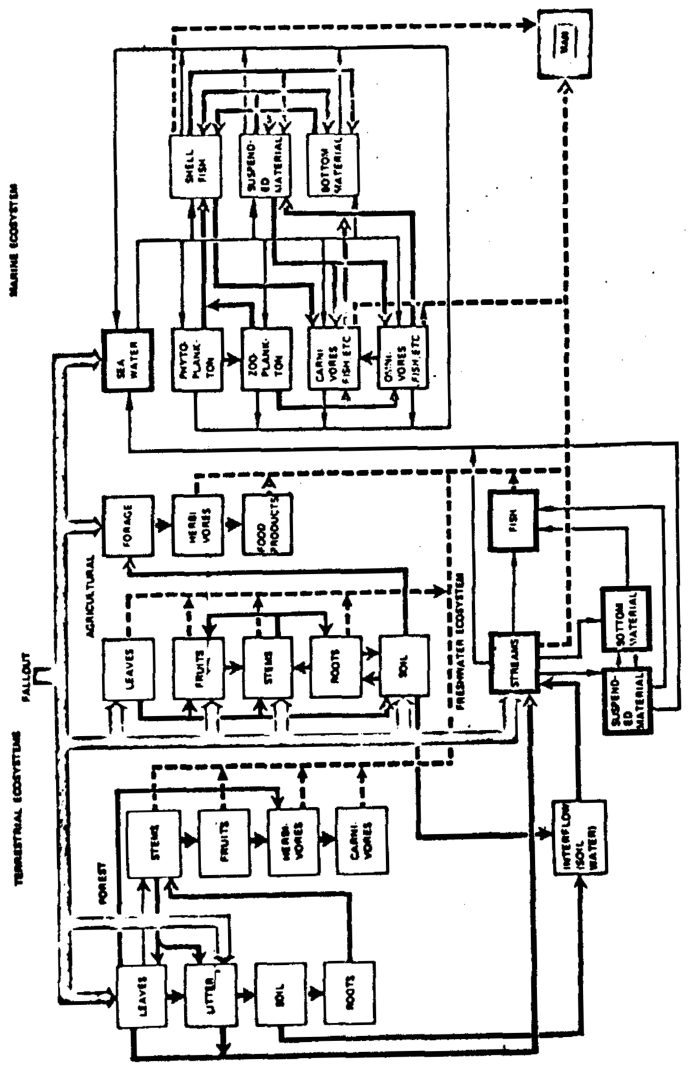

FIG.1 GENERALIZED RADIONUCLIDE-TRANSPORT DIAGRAM*

conservative, remedial action in an "uncontrolled" situation would usually not be justified until these dose limits are considerably exceeded.

It is not possible to specify universally applicable action levels because the health risk of the remedial action may vary depending on time and location. For example, a shift of dairy animals to stored feed may have very little disadvantageous impact if the farmers in a given area in a given season happen to have adequate supplies available; on the other hand, such a remedial measure might mean a complete cessation of milk production if stored feed was not available.

It has become evident that ^{131}I is the critical nuclide resulting from normal operation or accidental events in nuclear installations and during early periods after nuclear detonations. That is, unless ^{131}I reaches levels that require immediate remedial measures, it usually will not be necessary to consider other radionuclides. This is most fortunate, because ^{131}I is the one radionuclide that can be coped with on a public health basis without much difficulty. The circumstance that we cannot handle technologically on a large scale; namely, significant long-term ^{90}Sr contamination, seems unlikely to occur from any peacetime activity that can be envisioned.

VI. General Comments

The important thing about fallout is that we do have experimental data so that there is some degree of confidence in our ability to estimate radiation exposures to man and the biosphere from given types of releases. A main difference is that worldwide fallout is dispersed over the entire earth whereas peacetime uses for the most part produce localized contamination. This means that specific attention has to be given to local conditions. Also, attention has to be given to some radionuclides that were not important in fallout.

Some data, but little compared to fallout, are available in connection with underground uses of nuclear exposives. This information is best obtained for the purposes of the subcommittee from scientists who have been directly concerned with these projects.

VII. Summary

A. Mechanisms should be available so that large scale technological endeavors can be fairly assessed in terms of the overall public benefit before implementation; this is especially important when there is a biological cost and a using up of natural resources.

B. Although a great deal is known about environmental radiocontamination much more needs to be done to provide a factual basis for the above assessments. The areas in particular need of research include: 1) low level radiation effects on biological systems including man; 2) evaluation of radiation dosages to be received by critical parts of the biosphere from planned technical operations, 3) similar evaluation of non-radiation effects, 4) meaningful studies of risk-benefit relationships.

C. Recognizing that decisions will probably always have to be made before fully adequate data are available, concerned scientists should nevertheless see to it that as much data as possible are obtained and utilized. They should engage in public education so that government legislators and administrators, officials of private companies, scientists in other fields and the general public are made aware of the issues with balance and perspective. Scare tactics and irresponsible statements, while being of great news interest at the time, are not in the public interest since they may prevent or seriously delay the realization of needed benefits.

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Dr. COMAR. So, really, all I am pleading for is that we use the information that we have and try to take advantage of it. The point I made before is that many people don't realize that because of the mysticism of radiation and the problems of fallout and defense we have done a great deal more research in this area than we have with many other of the substances that we deal with.

I mean, there are nutagenic effects from various chemicals that have hardly been investigated to any extent. Yet, we spent millions and millions of dollars on radiation. So these things, I think, all have to be balanced. Perhaps I should turn to more technical matters and talk about environmental radiocontamination.

Generally our experience goes back to fallout. The situation is simply that one puts radioactivity into the biosphere and out of that comes some harm to society. What we require to know is the relationship between the input of radioactivity and the harm that occurs. Unfortunately, we can't make direct observations of harm because the levels with fallout have been so low that one just cannot see any detectable effects.

I have indicated a flow sheet where one shows that as radioactive materials are put into the environment, they become distributed through all levels of environment so one tries to determine the levels, how much cesium in the Alaskan Eskimo, how much iodine in the thyroid gland in children and so on, how much radioactivity in the various parts of the biosphere, and then one tries to get a relationship to the harm that that dosage might do in the biosphere and finally then tries to evaluate the harm that will occur to society.

This becomes a very, very complicated system as you can imagine, and the way it was handled in regard to fallout, the way it was reduced to manageable dimensions was to try to look at those factors that were called critical. The term "critical" was used to denote a factor that contributed in such a way to risk that other factors made no significant contributions.

In other words, if you find that you would not be worried about strontium or iodine then you wouldn't have to be worried about any other radionuclide. If the strontium levels or iodine levels are too high then one could focus attention on these particular nuclides.

Now, the general categories of critical factors include the organisms, the radionuclides, the pathways, the specific population and the specific tissue or organ. Let us talk about the critical organism first.

In regard to fallout the general consensus was that man was the only organism whose welfare had to be guarded. In other words, it was very difficult to conceive any situation in which the radiation effects on lower organisms could be observed unless these were at the same time an exposure of man that would be unacceptable.

I think as far as radiation is concerned that this would be a generally acceptable concept. Now for other types of effects, such as might be caused by thermal releases or earth moving operations, certainly we should look at the possibility of effects on other parts of the ecology, but for radiation, itself, I think if we are satisfied that man isn't being harmed our anxiety levels are set low enough that one would not be able to see any effects in the other parts of the ecological system. We had many interesting discussions on that point with our friends in ecology, as you might imagine.

About the critical radionuclides, I have indicated the radiation doses that are expected to be received by the total population from all nuclear tests that were carried out before 1968. I don't think we need to go through the numbers in detail, but I would make several points about them.

Senator GRAVEL. Let me ask a question. We had heard this morning that tritium wouldn't apply in this case?

Dr. COMAR. No. The contribution of tritium from fallout was so small compared to these other radionuclides that it just wouldn't be significant. There are values for tritium, but they are of the order of one hundredth of these values. Now, tritium as I will point out later, of course, does become important, but from weapons testing carried out today tritium makes a minor contribution.

These values come from the United Nations Scientific Committee and they represent collections of a great deal of data and the results of very sophisticated types of analyses.

Now, in terms of absolute levels, it should be noted that the natural background rate is about 120 millirads per year and the total dose for the tissue receiving the largest dose commitment over the 50 years or so, amounts to 240 millirads. Now, it is hard to make a comparison because of the time patterns, but over this 45 to 50 years, the population would be receiving about 5,000 millirads of background radiation and it is receiving at the most about 240 millirads from fallout so that we are talking about roughly a tenth of background as coming from fallout.

The one thing that isn't included here is the iodine–131 because that is very difficult to calculate on a population basis. One has to look at the local communities where this contamination occurred and make comparisons. I think it is interesting to look at these data to answer some of the burning questions that we were faced with several years ago, and these were the questions that are now similar to the questions we are facing here.

The first question was, should the dangers of fallout limit the testing of nuclear devices? I think the answer to that was no because of the benefits and because our need to have nuclear devices for our defense posture far outweighed the cost of adding a 10th background to a population dose.

Next, should we worry about the dangers of fallout? Should mothers try to use powdered milk instead of fresh milk and so on? Again, the answer is that Government should worry about the dangers of fallout and do what they can to follow the situation. But individuals would probably do more harm than good by worrying about fallout. They should have no more anxiety than the risk they accept in carrying out their daily living.

Unlike testing of nuclear weapons in the atmosphere, in power reactors and processing plants, there is a degree of control and the releases of radiocontaminants can be regulated to a considerable degree although obviously there cannot be 100-percent containment. There will always have to be, I presume, for economical operation, some release to the environment. Now, the same fission products in fallout are the ones that are involved in power production: iodine, strontium, cesium.

Then there may be other releases to the atmosphere. The important ones are crypton-85 and tritium. I don't know that I should go into details about these. I think they have been discussed before and we shall be getting hungry before long. In terms of nuclear devices for excavation projects, there are five categories of radioactive products: fission products, as we have discussed before; residual fissionable or fissionable material such as plutonium; thermonuclear reaction products such as tritium and beryllium 7; induced activities from device materials, and induced activities from surrounding materials.

I don't want to go into detail but there is in existence a Presidential commission that has over the past 5 years carried out an intensive study, has spent about $21 million trying to look at the totality of the Atlantic-Pacific Interoceanic Canal. Presumably, this Commission will make its report. I don't know what the report will say. That is still in preparation, but all of the aspects I presume will be looked at—the hazard aspect, the feasibility of engineering, benefit that one might expect to gain whether financially or politically, and I think that this would all be put together and synthesized in the report of this particular Commission and I assume that this would be of great interest to this particular group.

Let me say a word now about the critical pathways. In fallout the radioactivity fell on the land and the water alike, and because most of man's food is produced on land, it turns out that as far as fallout is concerned, the terrestrial pathway was the most important.

With regard to reactor outputs and excavation processes, other routes may become important, both the terrestrial, but more importantly the aquatic route may become of great significance, so that has to be taken into account.

A word about the specific population. It is always important in any aspect to be sure that no specific population group or individual is going to be exposed to unexpected levels of radiation by virtue of peculiar circumstances, and for this reason a very careful consideration was given to the situation of the Eskimos and the Laplanders in Scandanavia. It also turns out that in the United Kingdom there is a small critical group that happens to eat a particular kind of bread that is made from seaweed that happens to be grown near the effluent output of a reactor. Actually, the dose acquired by this particular group of bread eaters governs the operation of this particular reactor, so this points up that one has to look at each specific situation in terms of industrial applications.

With fallout one can make generalizations on a worldwide or countrywide basis, but for powerplants, and for excavations if they are done, I think one has to be very careful to look at the local situation to make sure that there are no problems.

The critical tissue: this is determined by the metabolism of the critical element.

We are talking about iodine which ends up in the thyroid, strontium which ends up in the bone, and cesium which ends up in the muscle, and the overall effect.

Let me say a word about remedial measures because this is something that seems to be misunderstood—the reason why remedial measures should not generally be applied when the general operating conditions are moderately exceeded.

Now, it seemed only reasonable and prudent that we should develop remedial countermeasures to mitigate, if needed, the effect of weapons testing and to be prepared in case of nuclear warfare and the general principles are available in the literature. But the most important point—and this is a very key thing—is that the health risk of the remedial action must be less than the risk from contamination at the action level. In other words, the remedial measure must do more good than it does harm. Now, at low levels of contamination that are in accordance with guidelines, the radiation risks are so small that almost any remedial measure that you would take would probably have produced more harm than the particular risk that you would avoid.

This is the basis for remedial actions requiring much higher levels before they should be put into effect. This is usually looked at in two ways: One looks at controllable operating conditions and these are usually planned and executed so that we don't exceed certain dose levels.

On the other hand, when we have uncontrolled situations, these have to be remedied by action against the contamination rather than by action against the source and depending on the circumstances then, it may be necessary to put into effect a remedial action.

Let me summarize at this stage. I think in general there should be administrative mechanisms available so that large-scale technological endeavors can be assessed in terms of the overall public benefit, and this is especially important when there is a biological cost and when these large-scale measures involve a using up or utilization of the natural resources.

If someone plans to build a power reactor that is going to utilize the coldness of the water of a given lake, then the people in that area have a stake in that and this matter should be looked at in total, and decisions made on the basis of the overall costs and benefits involved.

Now, we know a great deal about environmental contamination, but we need a lot more work. As always, we have to look at this business of low level radiation effects on biological systems including man. We still need more work on that although we will probably never get a direct answer. We have to evaluate the radiation doses that are going to be received by critical parts of the biosphere from any technical operation, we have to evaluate the nonradiation effects, that is the thermal effects; and I still feel that we must make very careful studies of risk-benefit relationships and this is very hard.

You know we always teach our students when you have an equation you have to have the same dimensions on each side, so you can't talk about risk in terms of saving lives and benefits in terms of saving dollars. Somehow you have to have the same dimensions on both sides but I don't think overall we have really done any great deal of research in this evaluation and I think this eventually has to be done or else we are going to be making decisions by capricious whimsy or intuitive feelings.

Now, I think we have to recognize, and this always turns out to be the case, that decisions are going to have to be made before fully adequate data are available. It is unfortunate but this seems to be the way of life. Nevertheless scientists should see to it that we get as much data as possible and, furthermore, that the data we do get is fully utilized.

I think that public education is terriby important. The Government legislators, the administrators, the officials of private companies, the scientists in other fields, the general public have to be made aware of the issues with balance and perspective and I feel again that irresponsible statements and scare tactics, while of great news interest at the time, may not be in the public interest, and I take recognition of your feeling and statements that we may sometimes have to have visible dissent to get over points not recognized otherwise.

Thank you.

Senator GRAVEL. Perhaps the most important point is that Dr. Radford raised, that we are decreasing our efforts in the study area on many of these facets through parsimony or what have you, while we pursue and continue the exploration of the other side. This I find very dangerous. I believe I interpret your statement to mean that you share our view that greater studies should be made in this regard.

Dr. COMAR. Yes. I have been worried about the decrease in research funds, the decrease in support for graduate students. I think that as our population grows and our needs grow, and developing countries need to have new technologies, we have to have new technologies, we have to keep pace with developments.

Of course, it sounds trite, like a cliché, to say we need more research, but I think we really must keep this effort going in order to keep pace with our problems in technology.

Senator GRAVEL. What happens in a case like this where we get a cutback in the whole area for an organization like yours which supplies information to the radiation council which has no funds to go in search of knowledge itself? Does this decrease the quality of advice or the depth of advice that you give to the council?

Senator GRAVEL. But if they are getting cut back?

Dr. COMAR. Yes. The National Academy committees, of course, are also nonoperational, but they have to draw the information from the work of scientists in general, from the laboratories of individual members of the committee, and for other scientists who publish papers.

Dr. COMAR. If they are getting cut back, then I am afraid we just don't have the input of basic information that we need.

Senator GRAVEL. Where is the accounting factor in this chain of advice with the radiation council? We get one step removed and again there is another advisory group with no operational funding. Now you tell me that your group goes back to the operational funding of individual scientists who undertake their studies, I would imagine, in a very pellmell way, what strikes their fancy of interest, or whatever their particular discipline is.

Then am I to infer that we have no planned program to acquire knowledge in specific areas that ask why we establish or how current the levels of radiation are?

Dr. COMAR. I think that we have fortuitously, but fortunately developed mechanisms for getting different types of research done. For example, we do have the national laboratories that are supported as in-house studies by the Atomic Energy Commission and in these laboratories a great deal of the programmatic and large-scale work is done. For example, the studies of Dr. Russell where we have something like a million mice under study to study the genetic effects so that we

have these national laboratories at Oak Ridge, Argonne, Brookhaven, to carry on the so-called mission oriented programmatic work that requires large facilities.

In addition, we have individual scientists at the universities who, for whatever reason they have, become curious about some particular phenomena and their work is usually on a small scale; in a sense their work is not planned but since there are a fairly large number of them and they do have this intellectual curiosity they have often come up with some of the basic ideas that then get fitted into the system.

Now, a big problem—and I don't think we want to get into it here—is really the funding of university research.

Senator GRAVEL. Right.

Dr. COMAR. And there are many implications here because unfortunately by historical accident the funding of university research has grown up through Federal agencies to a large extent, the Department of Defense, National Institutes of Health.

Senator GRAVEL. Apparently the only organized effort seems to be what is motivated and catalyzed by the AEC, itself. That is the interpretation I have made from your recent statement. Is this a correct interpretation?

Dr. COMAR. To a large extent I think that is true, although other agencies investigate and have interest in various types of radiation problems. For instance, the National Institutes of Health would be interested in studying medical uses of radiation. They would be interested in sponsoring the use of the atomic energy for powering artificial hearts and so on, so that each agency in its own particular interest would tend to foster and promote certain types of research.

Senator GRAVEL. But none would have the economic muscle within the Federal establishment of the AEC in determining a directional course?

Dr. COMAR. I think that is a fair statement.

Senator GRAVEL. Then is this a correct conclusion? We are told that the AEC depends for its standards upon the Federal Radiation Council and then when we delve more deeply and we find out that the main source of in-depth information is what is prosecuted by the AEC and that is what the Radiation Council gets.

You indicated earlier in your statement—I don't know if it was an endorsement of this particular bill—but the concern that I have and many others have is that it doesn't seem logical that the AEC which prosecutes a program and a good program and one that should be prosecuted because we do need atomic energy, but that the agency that does this should also have the complete control of environmental safety.

Dr. COMAR. I would agree that certainly the fundamental principle is sound that the same agency that promotes and produces should not be the agency that regulates. Following the practices of British diplomacy, one has to follow high principle by sound compromise, and I think that a large difficulty has been the problem of qualified personnel.

The atomic energy program since the beginning started out with a very specific technical specialty involved, and I think it is fair to say that practically all of the people, certainly in the early years who were

involved in any aspect of nuclear science, had some relationship to the Atomic Energy Commission, and I think with time this knowledge will become more diffuse and even now we have inputs from people who have had very little relationship with the Government, with the Atomic Energy Commission, so there may come a time when the technical abilities will be available so that these functions can be separated without difficulty.

Senator GRAVEL. Apparently, you say there may come a time, but you don't feel that the time is here to assume this activity?

Dr. COMAR. I really wouldn't like to make a judgment on that. I think that these matters sort of develop when an appropriate time comes. It wasn't so long ago when some of the responsibilities were turned over to the Public Health Service, and I think there were difficulties at the time that the Public Health Service, in all deference to them, did not have the appropriate personnel to carry on the responsibilities at the time. They have sort of grown into that, and at some time I think the other step would be made, but I wouldn't like to say when that will be.

Senator GRAVEL. Would it not seem logical if we wanted to pursue very actively the benefits of atomic energy or if we wanted to accelerate them, I personally feel, that we would accelerate the activities of the AEC because a group outside AEC could concentrate on one facet of the many areas that may be dangerous and monitor it.

I think you have answered the question though.

Dr. COMAR. I think we certainly agree in principle. I suspect it is a matter of time that is involved and I think you put your finger on an important point, and this is the business of reestablishing and maintaining the credability with whoever is involved with the standards, with the regulations or the licensing.

I think that we must have public confidence in the people who are carrying on those functions.

Senator GRAVEL. Of course, and I couldn't agree more. That is the reason why we feel that in order to assure this confidence the activity should be before us.

You mentioned on several occasions the Eskimo situation. Could you provide me with a copy of the Eskimo studies as to the affects of the radiation? It is a study, I believe, from some time back concerning the Arctic Eskimos, and the degree of contamination they experienced. Are you familiar with that?

Dr. COMAR. Yes. I suspect there are several publications of that data. I am sure these are available and we could make them available easily to the committee.

Senator GRAVEL. Thank you. The staff will examine them and decide as to whether or not we can print them.

(Subsequently Dr. Comar submitted seven scientific papers that were placed in the committee files for its use. They are as follows:)

"Fallout Radionuclides in Northern Alaskan Ecosystems," by Wayne C. Hanson, BS, Richland, Washington.

"Studies of Fallout ^{137}Cs in the Canadian North," by Peter M. Bird, Ph D, Ottawa, Ontario, Canada.

"Radioactive Food Chains in Subarctic Regions," by Jorma K. Miettinen, Helsinki, Finland.

"The Accumulation of Fallout Cesium-137 in Northern Alaskan Natives," by W. C. Hanson and H. E. Palmer, Richland, Washington.

"Cesium-137 in Freshwater Fish: Human Consumption and Body Burden Considerations," by P. F. Gustafson.

"Cesium-137 and Potassium in Finnish Lapps and Their Diet," by J. K. Miettinen, A. Jokelainen, P. Roine, K. Lidén, and Y. Naversten.

Senator GRAVEL (presiding). We will adjourn now, but we will probably provide you with a series of questions that the Senators may have to which you might respond.

(Subsequent to the hearing the following letter was sent to Dr. Comar:)

JANUARY 23, 1970.

Dr. CYRIL L. COMAR,
Professor of Physical Biology, Cornell University,
Ithaca, N.Y.

DEAR DR. COMAR: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass over any of the attached which are outside your area of specialty or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(The questions and answers appear in appendix IV.)

Senator GRAVEL. Thank you very much.

Dr. COMAR. Thank you.

Senator GRAVEL. Tomorrow we will have Dr. Robert Platt and Dr. Lamont Cole, and we will reconvene at 9:30 in this room.

(Whereupon, at 12:10 p.m., the committee adjourned, to reconvene at 9:30 a.m. the following day.)

UNDERGROUND USES OF NUCLEAR ENERGY

THURSDAY, NOVEMBER 20, 1969

U.S. SENATE,
SUBCOMMITTEE ON AIR AND WATER POLLUTION,
COMMITTEE ON PUBLIC WORKS,
Washington, D.C.

The subcommittee met at 9:50 a.m., pursuant to recess, in room 4200, New Senate Office Building, Senator Edmund S. Muskie (chairman of the subcommittee) presiding.

Present: Senators Muskie and Boggs.

Also present. Richard B. Royce, chief clerk and staff director; M. Barry Meyer, counsel; Leon G. Billings, Richard D. Grundy, professional staff members; Tom Jorling, minority counsel; and Walter Planet, Department of Commerce Fellow.

Senator MUSKIE. The committee will be in order.

We will resume our hearings which started 2 days ago. Our first witness this morning is Dr. Robert Platt, chairman of the Department of Biology of Emory University. Dr. Platt, it is a pleasure to welcome you this morning. I see you have company. I wish you would identify your colleague for the record.

Dr. PLATT. This is Dr. Lamont Cole.

Senator MUSKIE. Yes, I understand he will follow you.

It is a pleasure to welcome you both, gentlemen. You may proceed in your own way, Dr. Platt.

STATEMENT OF DR. ROBERT PLATT, CHAIRMAN, DEPARTMENT OF BIOLOGY, EMORY UNIVERSITY, ATLANTA, GA.

Dr. PLATT. Thank you, Senator Muskie. It is a pleasure to be here and I appreciate this opportunity. As I have pointed out in my statement, I am familiar with the excellent work that you are doing and I will proceed on this note of understanding.

Senator MUSKIE. Thank you, sir.

Dr. PLATT. I would like to say first that I strongly endorse this bill. I think it is an exceedingly important bill because it is a step in bringing into better coordination and focus our technology with our environment. What I would like to do, with your permission, is to simply go through the prepared statement and emphasize certain points as we go along.

Senator MUSKIE. That will be very helpful and we have all morning to do this.

Dr. PLATT. Thank you.

The statement is really in four parts. The first part deals with a broad perspective relating to technology and the environment. The first part has to do with a broad concept of our relationship of technology with environment, or human ecology, as I would like to refer to it.

In the second part I try to characterize the field of radiation ecology, the interplay of ionization radiation with man's environment.

In the third part I come down to that area in which I have been working for many years, the direct effects of radiation on the environment and then finish with some recommendations.

Senator MUSKIE. Fine. It sounds like a good course.

Dr. PLATT. Now in terms of placing our present situation into perspective, I like to think about it in this way. We can think of our western civilization as a great pendulum moving through space. This pendulum over the recent years has been pushed harder and harder by the forces of the industrial revolution, the atomic age, and the space age at really dizzying rates. And if we proceed as we all recognize, at the rate at which we are going our technology will carry us over the brink to where man may or may not survive as a species on this planet.

Now in order to brake the speed and momentum of this pendulum and to bring it back into some reasonable balance where man can live in harmony and happiness with his environment and himself, it seems to me that the opposing forces of human ecology must come into action—that is an awareness that man is part of his environment and he must live with it; that he must make technology serve society rather than society serve technology. This calls for close cooperation, of course, between the two areas.

Now what about the ecological forces? We now know that man, like all other forms of life, has evolved in harmony with his environment and that evolution is simply the consequences of the interplay of life with environment.

So man has a biological environment, but we seldom think that it has only been a little over 100 years that man knew he was part of this biological environment, this beginning with the theory of evolution by Darwin. Of course we still haven't fully accepted this fact.

So it has only been about 100 years, at least in western civilization, that man did not believe he came from the Garden of Eden in some form of special creation. This has put man into a perspective with his environment in which he assumed he is set apart and above and is master of his environment, and that his destiny is to wrest control of his environment and to emancipate himself from it. I think this has characterized much of our thinking.

Ecology, interestingly enough, did not get started in a sophisticated way until the concept of evolution was well developed. Early efforts at ecology were related to adaptation and to evolutionary processes. It has only been 50 years that scientists began to look at communities of organisms living together and their interactions with each other.

It has only been 15 years, approximately, that man has been able to look at a total life environment system in a sophisticated way. As you know, we define a total life environmental system as an ecosystem. And we have primarily man-managed ecosystems like cities and agricultural fields, or ponds, lakes, forests, that are more or less natural ecosystems.

It has been less than 10 years that there has been a public awareness of what man is doing to his environment. So we began to ask in the early sixties what are we doing to our environment, because the insults were getting so obvious that we couldn't avoid this question.

I would say 4 or 5 years ago we turned this question around and began to ask, What is the environment doing to man? This put us into a whole new ball game, because now we are the central figure. We can no longer have the detached view that we are doing these things to the environment and that is too bad so let us clean up the environment and have a better place in which to live.

When we turn it around and ask what is the environment doing to man in terms of environmental health, in terms of living space, and in terms of quality and supply of food, then each person becomes centrally involved in the whole issue.

When we look back in history I don't think we can put the finger on any one situation or any one thing which has brought us to the situation to which this bill is addressed. It is something that has developed from the moods of the country and our system of priorities, the emerging state of ecology and our awareness of the environment and of its significance to man. What we are concerned with though, is that now we have the concepts and the information on which we can act.

So not looking back but looking into the future, as this bill does, we have now an obvious and direct responsibility to bring our concept of environment into some balance with that of technology and to look at both as a total system in terms of man's welfare.

One of the interesting aspects of this bill is that we know very little about the effects of radiation on man's environment. Here the disparity between technology and environmental knowledge is an enormous gulf.

Now if I may turn to the second part, I would like to talk specifically to the field known as radiation ecology. I am restricting it to include ionizing radiation only since visible or ultraviolet radiations are other forms. I am talking, then, about the context of your bill, ionizing radiation.

This field of radiation ecology is normally divided into two parts. One is radiation effects ecology. The other is radioisotope ecology. Radiation effects ecology is concerned with the direct effects of ionizing radiation as an environmental stress on organisms and on communities of organisms and on total life-environment systems or ecosystems.

In this respect we can think of ionizing radiation as any other stress such as drought or famine. If one looks at catastrophic stresses, and this is the context in which the field of radiation effects ecology originated, there is overwhelming evidence that it does operate in this way.

Radioisotope ecology is concerned with the distribution and fate of radionuclides in the environment. It also has to do with the bioaccumulation of these radionuclides as they are moved through food chains and webs to man.

Much of the evidence in the last 2 days, along with Dr. Cole's, is

directed toward radioisotopes, while the evidence I would like to give later on refers to radiation effects as such.

During the first decade of the atomic age man was concerned with the environmental effects of radiation, but the field was very difficult to get a handle on. Around the air detonations, the heat and blast overrode the effects of direct radiation as such, and thus prevented any kind of definitive cause and effect studies to be carried out. We were concerned at that time also about the local and worldwide distribution of fallout and much was learned of course about this at atomic test sites and other places.

It wasn't until 1955, only 14 years ago, that the first organizational step was taken to develop a body of workers and sets of data and so on for this field. This was done by the Atomic Energy Commission through their Division of Biology and Medicine by setting up an office for Environmental Science. In 1958 this office was changed to the Environmental Sciences Branch of the Division of Biology and Medicine.

There weren't but a handful of workers at that time. And these were mostly on AEC *onsite* laboratories. Ecology itself was not in a particularly strong position. Many of the developments which have made ecology a very solid quantitative experimental science today were just in their beginning stages.

Two things that ecology now relies on is computer technology and the use of radioisotopes. Computer technology enables the ecologist to handle a large number of variables in a rational and intelligent manner. In 1955 we didn't have this capability. With most of the environmental variables largely uncontrolled, it is a difficult field to work in, particularly when compared to the brilliant accomplishments of molecular biology, where rigorously controlled laboratory experiments could be carried out.

The use of radioisotopes has given ecologists a means whereby they can trace metabolic pathways through life environment systems or ecosystems. This has led, of course, to much of our knowledge about the bioaccumulation of isotopes and it also has broad implications in determining the way that nature handles pesticides and other pollutants.

It is quite natural then that most of the knowledge about the effects of ionization radiation in the early years of the atomic age was obtained through molecular biology and was directed primarily at man himself, through its influence on human health and genetics. At that time there was also great interest in the effects on the use of radiation in creating new races of plants and animals that would be of some advantage to man.

There were no guidelines to follow in the field of radiation ecology. We really started from a position of practically zero, except of course, for the body of ecology which supported it, but I am talking about the specifics of radiation ecology as such.

It was very difficult in those days to use ionizing radiation experimentally in the environment. Enough wasn't known to trust its use away from onsite AEC laboratories.

I pointed out in my statement that I can recall some of the first experiments on irradiating a natural system. In my laboratory we put

segments of small ecosystems in greenhouse flats, put them on a cart and rolled them under an X-ray tube.

The first organized meeting of radioecologists didn't take place until the latter part of the fifties. Through the Environmental Sciences Branch of the AEC and through the interest of many of the ablest ecologists in this country, this very challenging and demanding field continued to grow. By 1961 there was enough known for the First National Symposium on Radiation Ecology to be conducted at Fort Collins, Colo. There were, as I recall, 91 papers by about 100 investigators who represented the field.

In 1967, as shown on page 9 of the statement, a second national symposium was held at the University of Michigan. By this time the number of people involved and the number of papers were approximately doubled. Another symposium, restricted to the terrestrial effects of radiation, including both radioisotope and radiation effects ecology, was held in 1965.

It is interesting that these three symposia pretty well summarized most of what we had learned in the areas concerned specifically with the environment, and excluding medical and cyto-genetical effects obtained primarily through the fields of molecular biology.

Two special programs of particular interest to this committee were initiated through the past decade. One was a comprehensive ecological study of an area in Alaska at Cape Thompson where Project Chariot was scheduled to take place. This was the excavation of a harbor. Over a period of about 3 years from 1958 to 1961—a team of 40 or 50 scientists—geologists, ecologists, botanists, zoologists, sociologists, and others, worked together to attempt to determine ahead of time what the effect might be of this proposed Plowshare event. Later as you know Project Chariot was cancelled.

Senator Gravel, I'm sure, is familiar with the publication of a large volume containing the results of these studies. Later on in the mid-sixties, a second, but far more sophisticated study was undertaken in the Rain Forest at the Puerto Rican nuclear center. Here in the Rain Forest an experimental radiation source was placed in the top of the forest canopy and left for a period of time so that the surrounding vegetation, plants and animals could receive radiation, and the consequences of this were evaluated.

Some hundred scientists have been involved with this program under the leadership of Dr. Howard P. Odum. A volume on the work is just about complete and should be published by the end of this year. This is a system approach to a large natural area with respect to radiation ecology and serves as a model for the kind of work which should be done later on.

Today, then, we have a manpower pool in this field of radiation ecology of about 300 very able scientists who have good rapport with each other and who are actively working, most of them in radioisotope ecology and a lesser number in radiation effects ecology.

I think it is interesting that this work has been supported almost entirely by the Atomic Energy Commission. It has also been supported at a very high level of objectivity and most of the data have been published in the open literature.

I conclude this section on page 10 by stating that in my personal

opinion a small group of dedicated Government and university scientists have fought a vigorous and uphill fight to obtain basic data. Until quite recently this area has stirred little interest by the public, by science-at-large, by various agencies, or by the Congress.

Again, this is the fault of no particular thing. Rather, it is a consequence of the way our national moods and sets of priorities and so on have developed.

I have listed some of the principal sources of information. I might add that extensive bibliographies have also been prepared. I suspect there exists today several thousand papers published in a variety of journals which deal with this specific area. Again I would like to emphasize that radiation ecology is basic ecology, and whether one uses ionizing radiation or radioisotopes, he is looking at the basic processes of life and environment.

I would like to turn next to the direct ecological effects of fallout from underground nuclear events as shown on page 12. This, interestingly enough, reports on about all that we know about the specific effects of fallout as such, for we were unable to do studies in this particular area until quite recently as I will explain later.

On the other hand, a substantial body of knowledge has been developed on radiation effects by using experimental sources such as cobalt or cesium, to obtain information on the radiation sensitivities of organisms and of ecosystems. We have done this in my own laboratory. We have a large, 2,400 curie cesium source in an open field, well protected, of course, and electronically controlled, where large segments of the environment or particular organisms may be irradiated.

A very interesting series of studies have been done at Brookhaven National Laboratory where a large ionizing radiation source has been placed in a natural environment for several years. At the Savannah River Laboratory a portable source has been developed which can be moved into an open field or into a forest where radiation may be released and the consequences studied. This is what happened of course at the Puerto Rican Nuclear Center for the rain forest study.

Another interesting input into this area was a situation afforded by an air shielded reactor built by Lockheed Aircraft Corp. for the Air Force and installed in a 10,000-acre reservation in north Georgia. The interesting this about this reactor is that, being air shielded, which means there is no brick or concrete or lead about it, radiation was released over several hundred meters away from the source. By good fortune this radiation was released in two bursts—one in June of one year and one in June of the next—and the duration of exposure was 2 to 3 weeks. Thus, we had an opportunity to look at a simulated situation as far as fallout is concerned, in a several hundred-acre field. Much of that work has been summarized in one of my papers and I have added that paper as an appendix. Mr. Grundy has a copy of it.[1]

Senator MUSKIE. Yes. It will be included in the record later.

Dr. PLATT. Now, moving specifically to fallout as such. As I pointed out earlier, the detonations in air of nuclear events produced so much heat and blast that it obscured any direct effects of radiation. This made it impossible to look at this aspect of fallout effects.

[1] See appendix.

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But in recent years a series of Plowshare events at the Nevada test site, where the shots have been underground but vented in the sense that a large crater was dug and a small fairly well defined fallout cloud was produced, has given us an opportunity to look at the effects of fallout per se on plant and animal life and on ecosystems.

This data is based on three of these events: Palanquin which occurred in 1955, Cabriolet which occurred in 1957, and Schooner which occurred last December, I would like to add that this program is being rapidly expanded. Sturtevant is the next event scheduled in which we will have a large input and I hope many more will follow.

Let me call your attention to the photomosaic which follows page 15, please. These are aerial photographs of Palanquin and Cabriolet. The details of the crater are well defined and the ejecta surrounding the craters shows up as white, being a loose rubble of rock and dust. You notice that the wind direction was to the north and the radioactive materials were carried downwind with a sharp line drawn at the edge of the ejecta on the upwind side.

We were able, by backtracking on Palanquin, because we began this study a year after it had occurred, to define three zones of damage. Subsequently we did the same for Cabriolet. Zone 1, close in to the crater, is the area where everything was killed. Of course, the doses close to the crater were many times greater than that necessary to kill the vegetation, so this is beyond the line of experimental study.

Zone 2, however, is that area where some organisms were killed, some injured, and some perhaps left intact, depending on their relative radiation sensitivities. So this is an area of particular interest experimentally.

Zone 3 is the area where there was minimal or no observable damage. Beyond zone 3 the levels of radioactivity were such that there were not only no observable effects but our experimental techniques would make it difficult to isolate or fix upon those that did occur.

Several interesting things have come from this series of studies. First, in analyzing the relationship between the beta component of the fallout and the gamma component, it turns out that the beta has a much higher concentration and in effect is the principal cause of damage.

For those listening who may not recall the distinction between beta radiation and gamma radiation, these being the two principal kinds emitted from fallout particles, I would like to give this brief distinction. Beta radiation has minimal penetration so that if it falls on the skin of a human, for example, it may produce blisters on the skin but would not penetrate sufficiently into the body to strike any of the vital organs. So the skin is an adequate shield. Gamma radiation on the other hand is much more penetrating, X-rays being a form of gamma radiation.

Why is it that during the atomic age, which began in 1945 and continues to the present, with nuclear events regularly scheduled through all of these years, that not until 2 or 3 years ago was it known that the beta component of the fallout was the critical part, at least with respect to direct effects.

One reason is that in the early studies where animals were parked around the events and where simulated work was done in the labora-

tory, it was determined that beta radiation would not penetrate human skin with sufficient depth to cause damage. The argument then went that since the gamma rather than the beta radiation would kill a human, there was no point in further study of beta with respect to direct effects because our philosophy at that time was that man is the critical factor. I think the difference between now and then is that now we know that the environment can exert more subtle and even drastic effects on the human than immediate effects. But that wasn't evident at that time.

Second, the technology for beta dosimetry had not been developed.

Third, the opportunities for this kind of study were difficult to come by. A visit was conducted by the Environmental Sciences Branch of AEC in 1959 to the test site to plan a biological shot so that these kinds of studies could be initiated, but the biological shot never came off.

A second major result of these studies is that plants, many invertebrates, and microorganisms are extremely susceptible to beta radiation. A plant bud is protected by a thin epidermis or at the most by a few bud scale leaves. If fallout collects on or around that bud or if there is a field of beta radiation surrounding it, then the sensitive tissues inside are readily penetrated.

An interesting aspect of this, is the ability of plants to act as fallout catchers. A shrub like Artemisia, which is the desert sage, has an intensely woolly leaf and toward the end of the twig, the leaves are *tightly compacted*. This combination makes an excellent fallout catcher. On the other hand, Juniper, the dominant shrub of Great Basin Desert vegetation, has smooth, slick branches. Fallout, of course, is shed very quickly from them.

The part of the data which substantiates the beta-gamma ratios from fallout is given later in this paper.

There are two interesting things about these graphs. The top gives the gamma dose in Rads. I might say that for this study extensive series of dosimeters were developed especially for this purpose. Some of the stations which were set up prior to the detonation of Cabriolet are indicated from left to right. There are three bars for each upright graph, the white one representing a dose received on the front of an Artemisia or sage plant, the middle one the dose received at the top, and the black one the does received on the back side. The plant itself then acts as a fallout catcher because with the wind blowing across the plant more particles will be intercepted by the front part than by the back part.

Secondly, the doses vary across the pattern from one edge to another and of course they vary with distance from the source.

Now notice that the gamma dose in Rads has a maximum figure of 794 at the point where we measured. On the other hand, the beta dose, on the bottom graph, goes to about 7,000 Rads. The ratio of beta to gamma varies from about 14 to 1 at these higher doses, to about 4 to 1 at the lower doses.

Now, may I call your attention to a table following page 20. I don't want to explain the whole table but I would like to point out certain things about it. This happens to be the first instance in which these many different kinds of data can be brought to bear on one particular situation.

The first four items in this table have to do with predicting radiation effects on particular organisms. These data have been developed primarily at Brookhaven National Laboratory with Arnold Sparrow and his colleagues in this country and abroad, and they represent what has come to be a very sophisticated and very effective means of predicting the radiation sensitivity of a particular plant.

This does not apply to animals. It is based on the fact that the larger the nucleus and the greater the number of chromosomes per cell, the greater number of "hits" the nucleus and the chromosomes will get in a given situation. Therefore, the greater the damage.

If I may paraphrase it this way for the sake of explanation, radiation showering down or tearing through the tissues, so to speak, leaves ionizing pathways which cause breaks and other damage. A small target would receive fewer of these than would a larger target.

Therefore, if one knows the nuclear volume in item 1, and/or the interphase chromosome volume as in item 2, then it is possible to predict what level of radiation would be required to produce a certain level of damage, such as LD_{100}. LD_{100} is the 100-percent lethal dose, and LD_{50} would be the 50-percent lethal dose.

Now, based on the nuclear volume in the interphase chromosome volume of these desert plants of that ecosystem, items 3 and 4 give the predicted doses to produce 100 percent death.

Looking at the left-hand column on Artemisia, in item 3 it would require 1,700 rads, if one assumes an interphase chromosome volume of 26.5—there is some question as to which of these volumes is correct. This can be determined, but it has not yet. Or if we take the lower figure, then it would require 2,700 rads to produce 100-percent death.

There is also a relationship between dose rate and effects. By this I mean that if the dose is delivered within a few seconds or few minutes or few hours, it would have greater effect than if the same dose was delivered over a few months time. Thus, the more intense the dose rate for a given dose, the greater the damage.

Therefore, if we change item 3 from 16 hours exposure for a given dose to that from simulated fallout decay in which the dose would come within the first few hours rather than a longer period of time, the amount of radiation required to produce a given effect is reduced. Now these are our predictions.

The next three items show what happened. In order to get a relationship between the sensitivity of these desert perennials and that obtained from plants and animals across the country by experimental sources, we hauled a 10,000 curie gamma source to an area near Palanquin and left it exposed for 34 days, thus accumulating doses from 100,000 rads to background levels. It turns out that it requires under these circumstances 30,000 rads to kill Artemisia instead of the predicted 1,700 or 2,700.

Now if we reduce the 34 days' exposure back to the acute dose expected from fallout, for which 75 percent of the dose will come in the first 12 hours, the lethal dose might be reduced to 10,000 or 15,000 rads but even with this, there is a great discrepancy.

Now going to item 7, where we were able to put dosimeters in the field prior to an event, it required a total dose of about 5,500 rads to

produce 100 percent death, of which the gamma component for Artemisia was 543 and the beta component 5,000.

Now what can we conclude from these discrepancies and these data? They indicate that our ability to predict in the field from data obtained in experimental circumstances is not very good. The state of the art is still at a relatively low level of development.

One of the reasons for this is that much of our empirical data has come from controlled experiments in the laboratory. It is difficult to extrapolate from these what is going to happen in a given situation in the natural environment where there are many stresses, a lot uncontrolled and many of them unknown, interacting with the irradiated organism. Furthermore, each natural system varies so that a desert ecosystem would not have the same characteristics as the tundra or a spruce forest the same as the Rain Forest.

To make these data really meaningful, experiments must be carried out in the field at the location where the expected radiation exposures will take place. The other thing that this emphasizes again is that it is the beta component of fallout which is significant.

I would like to parenthetically discuss the significance of the beta component of fallout in terms of nuclear war. Most of our criteria for determining the effects of a 20,000 megaton attack on this country have been based on what we know about the gamma component of fallout. We have not put into these equations and judgments the beta component. So these data suggest that our whole area of anticipated environmental effects of a nuclear war be reevaluated.

A number of other things have come from these studies and I am not going to review them now. I think I have made the principal points except one—that is recovery. Ecosystems, like the human individuals, have the capacity to absorb and react to stress and to recover from stress. How long does it take an ecosystem to recover from a devastating stress such as flood or fire or ionizing radiation? This depends, in part, on the climate. In the desert, it may take several hundred years, where the growth rates and conditions for physiological activity are very difficult. In an agricultural field where man can assist and the soil is intact, it may take a much shorter time. If one cuts a deciduous forest, it may take 100 years for that forest to reestablish itself under natural circumstances.

If man so overthrows the system that most of its homeostatic balances are gone, it may never recover and there are many instances around the world where this has happened. An example is in England where the forests were cut away centuries ago for firewood and they have not become reforested. The ecological balance which permitted forests to develop there were already on the edge, and this traumatic event of cutting overthrew the balance of this system so that it has never become reestablished.

We do not know much about the recovery of ecosystems from radiation stress. We can extrapolate. We can say, well, under other stresses we know what would happen. If a fire sweeps through, how long does it take for the balance to be restored.

But it is dangerous to make definitive extrapolations as I have pointed out here because it just might not turn out the way one thinks it is going to turn out. Our experience with the direct effects of radiation has not been extensive enough or long enough to do more than make considered judgments on this point. It is something on which we need a great deal more information.

I make the point that society in all of its ramifications is a total system and that we cannot unilaterally consider certain parts of it without doing so in reference to others.

I think technology should work hand in hand with society. I suspect through the forces of technology and economic determinism this has not been the case. We have to, in my judgment, turn this around and ask the question "can technology serve society?" To do this man must be prepared to pay the increased cost of technology as necessary and he also must be willing to withhold certain technological applications until he can find out what the total cost of these applications would be to man.

In the field of radiation ecology we have not had the kind of cooperation that is now needed. Again, I don't put an accusing finger on anyone. I say this is just the way things have developed and I am not concerned about how or why we have arrived at our present situation, but I am deeply concerned that we rectify this.

On item 7 I would like to come back again to the point that there is one ecology. We can talk about radiation ecology, human ecology, pollution ecology, plant ecology and what have you, but they all relate to the same set of fundamental rules and interactions by which all of life is controlled.

Now item 8 turns to a somewhat different subject. That is a matter of public information. I am personally convinced, and have been for a long time, as have many others, that the public is poorly informed about the uses and abuses or the advantages and disadvantages of ionizing radiation. I don't believe we can really utilize atomic energy to our best advantage until the public understands, in a much more meaningful way, what it is all about.

This calls for thoughtful and constructive information. Perhaps this committee can go far toward accomplishing this objective. Some people feel that this is the principal roadblock today in our development of peaceful uses of nuclear energy.

Now, turning to my final recommendation, please. I have given in item 9 some of the areas of particular concern where we need information. The first is on sublethal effects from radiation doses of only a few rads, less than 2 to 300. These are sublethal doses where subtle effects are on basic physiological processes such as photosynthesis, reproduction, respiration, the various aspects of growth, and on the ability of an organism to compete with others in a community in order to maintain a reasonable balance. This is an area in which we need much information and also where many of the workers today are active.

As I pointed out, we now need to go back and work with beta radiation to complement the extensive work done with gamma and there are a number of very interesting experiments being developed now around the country on this point.

We know very little about the subsurface effects within the soil. And of course as I mentioned above, effects on physiological processes. We have almost no information on the effects of absorbed doses through

radioisotopes for other than a handful of organisms. We know what it will do to man if he picks up ratioactive iodine. That which is concentrated in the thyroid may produce cancer. We understand the effects of strontium-90 being accumulated in the bone marrow. We understand these things for a handful of organisms. But what does the internal doses from absorbed and accumulated isotopes do to the whole host of several hundred thousand different kinds of plants and animals which make up man's natural environment and on which he is absolutely dependent if he is to survive on this earth. This field is hardly touched.

Now I would like to put these concluding remarks in this perspective. We live in a crisis society and we have learned to react to crises. Environmental crises have been turning up with increasing frequency over the past 10 years and particularly over the past 5 years and just in the last few days.

We are attuned to put out the fires to meet the crisis. And we attempt to meet this with more technology. Antismog devices for cars, smoke abatement on chimneys, treatment of sewage disposal before it gets into the rivers, and we can go on and on.

This is the main intent, as I understand it, of our water pollution laws, our air pollution, and so on. We are trying to put out or subdue the crises which have presented themselves to us in such an overwhelming way that we can no longer ignore them.

Now, we must take one step further. I think we can safely assume—in fact I think we innately know—that there are going to be more crises and that we don't know what these subsequent crises are going to be. And until we set up definitive studies with regard to the environment, which will give us guidelines to go on, which will ferret out dangers before they emerge into a crisis position, we cannot intelligently face the future.

This is particularly true with reference to this bill. I don't see how we can continue to use nuclear energy just because it is a great thing and because it does good to society in one way when we do not have the knowledge and the ability to predict its long-range consequences to man. I understand very well that this is the purpose of this bill and it is the reason for which I very strongly support it.

Now, how can we accomplish this? One way is by the development of regional laboratories and by regions I mean biotic provinces, climatic provinces, throughout this country where teams of engineers and scientists in many fields can work together in looking into the basic ecology of the environment and developing the kinds of information which will make it possible to predict and ward off subsequent crises as well as help us to intelligently handle those with which we are now faced.

Thank you, Senator Muskie.

Senator MUSKIE. Thank you very much for your testimony, Dr. Platt. I especially appreciated your concluding remarks which I think do summarize the philosophy and the approach of this subcommittee and of this legislation. We are scrambling so hard to put out the fires already started that we don't focus as we should on trying to prevent fires that as you say are inevitable unless we begin to do something about them.

I would like to pinpoint for emphasis what I think are a few of your conclusions.

First of all you said several times in your presentation that we know very little about the effects of radiation upon the environment. Secondly, you have said that there is a need for technology and ecology to form a working union and that this is barely beginning to happen at the Nevada test site.

In other words, from your vantage point we are not now doing what we ought to be doing to evaluate the radiation effects of our radiation technology. And you focus this complaint especially upon the Plowshare program I take it.

Dr. PLATT. Yes, sir.

Senator MUSKIE. Is there any evidence again from your vantage point, of a determination to work in this direction on the part of the agencies of the Government who have the principal mission of developing, our nuclear technology for peaceful purposes?

Dr. PLATT. I would like to make two comments on that. First, the Atomic Energy Commission in its mission to understand the effects of radiation on environment has done all that it could under the existing circumstances of the past several years. The demands for these environmental studies of which I speak did not exist, the overwhelming evidence that they must be done just wasn't present.

Second, I would like to say that at the test site now there is an increasing interest in such studies particularly since we undertook this series 2½ years ago and have gotten such spectacular results in terms of the direct effects of fallout.

If I may make a third point, the momentum of technology and its majesty, and its enormous scope, and the power back of it is such that without a very, very strong effort to introduce these environmental studies at once, it may still be several years before we arrive at a satisfactory state of knowledge.

I don't want to fault the Atomic Energy Commission in this respect. I think they have done what they thought should be done. Ecologists have insisted we need more information but their voices have not been effective. We didn't have the hard data to make it effective but now we do.

So without looking back, but looking forward, we need to institute drastic changes in our system of priorities within the Joint Committee and within the Atomic Energy Commission.

Senator MUSKIE. Let me ask two questions bearing on what you just said.

First, you refer to the spectacular results of the work that has been done to date. By this do you mean to suggest that as a consequence of these results we ought to have greater concern about the effects upon the ecology and the environment than we had before?

Dr. PLATT. I think these results have not changed the basic issue but they have greatly reinforced them.

Senator MUSKIE. In other words, we ought to have a greater sense of urgency as a result of these tests than we had before, and by we, I am not referring again to anybody in particular. I am talking about the actual policymakers, the trustees of the public interest.

Dr. PLATT. Right. These data emphasize in a rather dramatic way

our deficiencies of knowledge in this area. I don't want to be unfair to my colleagues and I don't intend to be. The urgency of this work has long been recognized and well known and documented. What our work at the test site has done is to again reinforce in a dramatic way what we have known all along.

Senator MUSKIE. In your prepared remarks there is a statement you did not read which I think is worth reading, and it responds to the question I have just put.

You began with a question:

> Shall modern man abruptly fade from the face of the earth? By the atomic bomb, he gave himself a unique gift—the capacity for self-destruction in one great holocaust. Now his technological wastes have given him another but more exquisite means of self-destruction. Are we now returning from a very long era of futile and self-destructive efforts at conquering nature, to a realization that the quality of life is proportional to the harmony we can achieve with all aspects of our environment? A projection of our present efforts would indicate that this is true.

As the second question, I would like to ask if this is a fair representation of the magnitude of the callenge that the little knowledge we have poses for us that what is necessary now is the exertion of a high art of leadership to arouse public concern and to generate the research necessary for precise measurement of the impact upon ecology of our activities?

Dr. PLATT. That is correct. But— and I am sure we have rapport on this point—that it be a constructive and balanced approach and not a unilateral "isn't everything terrible" approach. We are looking at the total system of man and environment of which technology is an exceedingly important part. There is a technology of the environment itself, and ecology perhaps is another kind of technology which we need to bring into balance.

Senator MUSKIE. I naturally react with favor to this approach. However, I think a lack of confidence in our country's response constitutes a crisis of our times and that the failure of institutional leadership in the institutions to respond to news challenges with real commitment can provoke an irrational response to the challenges.

I ask these questions not to provoke any irrational response, but to stimulate, I hope, an ordered and concerned commitment that will move us in the needed direction in which we ought to move. I appreciate your emphasis on the need for rationality, but I think we ought to recognize also that governmental institutions have got to be more open about this than they instinctively are, and they ave to be jarred into an awareness, jarred into public recognition of the risk that they are creating, jarred into visibly assuming the responsibilities that I think the people are going to increasingly demand that they discharge effectively.

I don't think we have to say these things looking back to blame anybody. I tend to agree with you that we all share the responsibility for generating the present environmental crisis. We naturally assumed that since America is so blessed with an overabundance of resources, we could afford to waste a great deal without being concerned about

the impact on our ability to survive as race. Now I think we have reached the line where this attitude is no longer permissible.

The third question I wanted to ask is this: You said that it could take several years of study at a higher level of urgency and concern and commitment to really understand the impact upon the environment and upon ecology of this radiation technology. If that is so, what should be the impact upon our technological developments, upon the pace at which we push back the technological horizons? What should be the effect upon the pace of the Plowshare programs? Should we restrain it, or should we declare a moratorium on some aspects of it? Are we dealing with this information in these studies? Just how urgent is it from this point of view?

As you have said, we are so entranced by the benefits of technology that we proceed with their development with an awareness and a sense of the risk, but without a desire to postpone the benefits until we are sure as we ought to be about the risks.

Dr. PLATT. I think that we should ask the question very realistically. What are the advantages to society of continuing at our present rate of development of technology? And we may find objectively that there isn't much to be lost in slowing down or waiting, particularly on certain aspects, and on those a moratorium or drastic slowdown should be instituted. Except on those whereby some national emergency in terms of the defense posture of this country or some other overwhelming reason perhaps we should continue. These are value judgments which I think should be very carefully and objectively considered in depth by, as you suggest, a variety of people who represent the multiple interests of society.

Now, may I add one other point to that?

Senator MUSKIE. Yes.

Dr. PLATT. We send a man to the moon and we have several moon flights projected. Suppose we took the amount of money for one moon flight and put it into environmental studies? Suppose we cut out one supersonic bomber or suppose we diverted the money from a Plowshare event? From which then, would society derive the greatest benefit?

I think our system of priorities, of funding, needs a drastic revision on this point. I cannot think of any greater national emergency today than greater knowledge of our environment, but until now, this has not been given, in the sense that you and I are talking about it, a very high priority.

The difficulties that the international biological program is having is an example of this. The problems in funding basic science through NSF is another example. The relatively small budget for environmental studies within the division of biology and medicine is another example.

Let us put it this way: society is willing to bear the cost of sending a man to the moon or developing nuclear reactors, but society has not yet indicated its willingness to spend an equal amount of money in looking at the effects of a nuclear reactor on the environment, or the effects of digging a Panama Canal or other such peaceful uses of atomic energy.

Senator MUSKIE. It is conceivable that we may destroy our environment on earth before we create another one on another planet to which man can move.

Dr. PLATT. Yes, sir.

Senator MUSKIE. The Plowshare program has several areas of exploration, canal construction, hydropower, water resource projects, general transportation construction. Which of these could you pinpoint as areas in which we should exercise the greatest restraint in moving forward operationally for the development of applications?

Dr. PLATT. Well, the canal stands out as by far the most significant because it involves not only the excavation itself, but a radical disturbance of two of the great natural systems of our planet, the Atlantic and Pacific Oceans. Also, because of the people who live nearby, there are considerations of sociological and biological portent. So the implications of the Panama Canal are of great scope.

Senator MUSKIE. As you have said in your statement, one of the areas where we have very little knowledge is the subsurface impact of these explosions.

Dr. PLATT. By subsurface I meant within the area below the ground where life exists, which would be the upper few inches of the mantle of soil around the world. We generally assume that the earth absorbs much of the radiation dose and therefore effects on organisms which live below the surface of the ground are not as important to us as those above.

But this also has a long history because man is not accustomed to looking below the surface of the ground and he is unaware of the vast world of life which exists there. It is this world of life which makes agriculture possible, the production of natural growth, the establishment of a forest.

Senator MUSKIE. On much of this technology we have, we seem to be following a philosophy which is often crudely expressed as "what we don't know can't hurt us."

Dr. PLATT. Exactly right.

Senator MUSKIE. What you are saying is that what we don't know can be mortal to the human race, in this field especially.

Dr. Platt, thank you very much for your excellent testimony.

Dr. PLATT. Thank you, sir.

Senator MUSKIE. Since you only highlighted your prepared statement, I think it would be very helpful to have it in the record at this point.

(The statement is as follows:)

PREPARED STATEMENT OF ROBERT B. PLATT, PROFESSOR AND CHAIRMAN, DEPARTMENT OF BIOLOGY, EMORY UNIVERSITY

I appreciate this opportunity to appear before you as a private citizen and university scientist, with reference to Senate Bill 3042: "To provide for a study and evaluation of the air and water pollution and other environmental effects of underground uses of nuclear energy for excavation and other purposes.

I am familiar with the excellent leadership you and other members of Congress are providing in the broad area of Ecology, and I am deeply grateful for this. My introductory remarks are made on this note of understanding.

This is a Bill of extraordinary importance. First, it focuses on a critical aspect of this program—the adverse effects of underground nuclear events on our environment. But of even greater significance, through this focus, it emphasizes the

primary objective of the program, which in its broadest context is the use of nuclear energy for man's welfare. We are in the Nuclear Age. Man needs nuclear energy. The problem is how to use it to the best advantage of Society. I sincerely hope, therefore, that in the Hearings planned for the future on specific applications, the advantages as well as the disadvantages of underground uses of nuclear energy will be explored. The emphasis that this Bill places on environmental effects is most appropriate, for our knowledge here lags behind that of nuclear technology. Apparently the competence for excavation of canals, harbors, and underground storage chambers has been developed, but not enough is yet known about the full effects, good and bad, which such activities may exert directly or indirectly on man.

With your indulgence, I would like to put this into an even broader perspective. Man stands at the crossroad of success or failure as a biological species. Modern man, *Homo Sapiens*, appeared on this planet a scant 20–30,000 years ago and his immediate human predecessors only within the past million. Shall modern man abruptly fade from the face of the earth? By the atomic bomb, he gave himself a unique gift—the capacity for self-destruction in one great holocaust. Now his technological wastes have given him another but more exquisite means of self-destruction. Are you now returning from a very long era of futile and self-destructive efforts at conquering nature, to a realization that the quality of life is proportional to the harmony we can achieve with all aspects of our environment? A projection of our present efforts would indicate that this is true.

As a perspective, I would like to develop the analogy of our western civilization as a great pendulum whose speed and direction of motion through time is determined by the forces of technology and economic determinism on the one hand and the opposing forces of human ecology on the other. Both forces are needed to keep our civilization healthy. Our problem then is to find the means of restoring an adequate balance within the context of society as it exists today and as we desire it for the future.

The forces of technology and economic determinism, fired sequentially by the industrial age, the atomic age and the space age, have pushed the pendulum at an ever increasing and recently a dizzying speed. The opposing ecological forces began to effectively slow this motion about a decade ago, and within the past 5 years may be beginning to bring it into a better balance. We now have the knowledge, concepts and attitudes, not only to reverse this motion, but to then push the pendulum back to an equilibrium position which is favorable for human health and happiness. The unknown factor is man's will to do this.

I would like to trace the development of these ecological forces. If they are to reverse the swing of the pendulum, they must be more powerful than the desire for economic profit. Ecological laws and processes are as old as life itself, for they have shaped the evolution of all life including the most recent and dominant of the primates, man! But man, not knowing this, placed himself above and apart from all other life. Not until 100 years ago did he even begin to understand that his evolutionary origin was not a mere few thousand years ago in the Garden of Eden, but millions of years ago as another form of life appearing in the evolutionary parade of microbes, plants, and animals.

Man's scientific approach to ecological relationships also had its origins some hundred years ago. The early efforts, called natural history, were concerned with the evolutionary relationships of adaptation and geographic distribution. Less than 50 years ago another level of complexity was reached with the study of communities of organisms, such as occur in a pond or forest. Not until 10 to 15 years ago was man's knowledge sufficiently enhanced to permit a sophisticated study of an entire life environment system—the ecosystem. The kinds of ecosystems range from those dominated by man, such as cities and agricultural fields, to those less affected by him such as mountains and rivers. All of these make up one vast interacting ecosystem, the planet earth. The ecosystem concept has provided not only a powerful tool but also a catalyst for understanding the effects of our avalanching technology on our environment.

Armed with proven methods and facts, various segments of society are attempting to halt this avalanche. Through the first half of the 1960's, the major emphasis was on what effects is man exerting on his environment. Through the midpart, however, another highly significant development took place. The question was turned around. For the first time in our history the crucial question was being asked on a broad front—What is our environment doing to us? Whereas

the former permitted us to continue our age old feeling of environmental detachment, the latter hit us squarely in the face. How man knew that he was personally involved and his welfare was at stake. Our reaction to this question has been prompt and decisive. Almost every significant advance in ecological legislation in this country has occurred within the past 5 years. Each year, each month and each day, new ecological developments and relationships are recognized, as for example these hearings.

There is only one ecology. For the sake of emphasis and action, we think in terms of human ecology and plant ecology, or urban ecology and forest ecology, or of *transportation ecology* and radiation ecology. But all aspects of life environment systems are under the same set of basic laws and principles as developed through the two billion year old crucible of evolutionary process. The humanizing element, which sets man apart from other life, is that he has the capacity to *alter and manage these systems*. One ultimate measure of man's humanism is the extent to which he can do this in harmony with these natural laws.

Thus, this Bill as I understand it, is another step towards placing our present values into a better balance with all others. As such, it makes a significant contribution to the powerful humanizing force of ecology. Acceptance of this force might well be the greatest challenge man has yet faced.

NOTES ON THE DEVELOPMENT OF RADIATION ECOLOGY AS A FIELD OF STUDY

For this review, I am restricting the scope of radiation ecology to include only ionizing radiation. There are two fairly distinct aspects of this field, one being *radiation effects ecology* and the other *radiosotope ecology*. The former is concerned primarily with the direct effects of radiation, either from experimental point sources or from fallout, on species, communities, and ecosystems.

The latter, radioisotope ecology, is concerned primarily with the distribution and fate of radionuclides in the total environment, with emphasis on their metabolic pathways through organisms and ecosystems, and on their *bioaccumulation* through food chains and webs to man.

Although an understanding of the effects of ionizing radiation on cells and chromosomes from both external and internal emitters is central to radiation ecology, most of this work has been done by investigators in other fields. However, the manifestation of these cytogenetic effects through physiological processes such as growth and reproduction are, of course, an integral part of radiation ecology.

From another point of view, the contributions on radiation effects from molecular biology have come primarily from rigorously controlled experiments on laboratory tested organisms. The contributions relating to the natural environment have come primarily from radiation ecology.

My notes are based on my active participation in this field since its beginning about 15 years ago. During the first decade of the Atomic Age, the emphasis was on nuclear technology and on molecular biology with respect to medical and genetical effects and uses as noted above. Early ecological effects studies, such as those at the Pacific and Nevada Test Sites were not definitive since the stresses from the heat and blast of above ground nuclear events obscured the direct effects of radiation, while away from the detonation site, the doses from fallout were too low to produce observable damage. However, much of the preliminary data on local and worldwide distribution of fallout were obtained during this time.

The first organizational step in the development of radiation ecology was the establishment in 1955, of an office for Environmental Science within the AEC Division of Biology and Medicine, subsequently changed in 1958 to the Environmental Sciences Branch.

Only a handful of scientists were then interested in radiation ecology, and their work was being done mostly at regional AEC laboratories such as Oak Ridge, Savannah River and Hanford. These early researchers faced a Herculean task. The environmental factors were many and mostly uncontrolled. Some of the principal developments which have brought ecology into its present prominence as a solid science were barely underway. For example, computer technology by which ecology can now handle its many variables along with modern concepts of the ecosystem, were in their very early stages and little had yet been done about the use of radioisotopes in delineating metabolic pathways through natural systems. From another point of view there was no organized cadre of researchers, few guide lines to follow, little financial support, and the state of

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nuclear science was such that it was almost impossible to use isotopes away from AEC installations and laboratories. I recall that the first irradiation of a natural system was done in my laboratory in 1956 by placing small ecosystem segments in greenhouse flats on a table and rolling them under an X-ray tube.

The first formal meeting of radiation ecologists was held at the Savannah River Plant, by John N. Wolfe, who was then and continues to be Chief of the Environmental Sciences Branch. Among the first papers to attract widespread scientific and political interest was by John N. Wolfe in 1959 on the Ecological Effects of Nuclear War. H. H. Mitchell of The Rand Corporation presented in 1961 perhaps the first civil defense analysis of ecological problems relating to post-nuclear war recuperation.

The First National Symposium of Radioecology was held in 1961 under the auspices of the American Institute of Biological Sciences and Environmental Sciences Branch of AEC. By then, the field had developed so that ninety one papers were presented by about 100 investigators.* A Second National Symposium was held in 1967 under the auspices of the Ecological Society of America, the University of Michigan and the *Division of Biology and Medicine*. AEC. By this time the number of papers and investigators had approximately doubled, but in each instance the symposia included most of the workers and research projects of their times.

Another landmark was publication of "Environment of the Cape Thompson Region, Alaska", composed of 41 papers and edited by N. J. Willmouvsky and John N. Wolfe. This is a highly sophisticated and comprehensive description and interpretation of an area in northwestern Alaska that had been selected as the site of a possible experimental excavation of a harbor with the use of nuclear explosives. The investigations were made mostly between 1959 and 1961. The work was guided by a small committee from the AEC and from other areas covering a wide range of disciplines. Later, the proposed excavation, known as Project Chariot, was cancelled.

Today the manpower pool of radiation ecology consists of about 300 very able and interacting scientists. The majority of these are in colleges and universities and most of the remaining in Atomic Energy Commission onsite ecology laboratories, and a smaller number with U.S.P.H.S. The field is healthy, with a continuing infusion of new ideas and approaches.

Financial support through the years has come primarily from the Atomic Energy Commission, including more than 90 percent of the support for radiation effects and more than half for radioisotope ecology.

The initiative and freedom of academic investigators has been well protected. The criteria for AEC support has been whether the research, as conceived by the investigator, contributes in a very broad way to the mission of the Atomic Energy Commission, and whether it is sound research. Almost all of the results have appeared in the open literature.

In my personal opinion, a small group of dedicated government and university scientists have fought a vigorous and uphill fight to obtain basic data on the environmental effects of ionizing radiation. Until recently, this area has stirred little interest by the public, by science-at-large, by granting agencies, or by the Congress, due to a combination of national moods, changing sets of priorities, the emerging stature of ecology and other similar factors.

Thus, today, before we move mountains, excavate manals and otherwise alter the global environment, there is much to learn about what these achievements will do to man and his environment in terms of safety, welfare, and total ecology.

Some pertinent references: Publications in this field now number in the thousands, and are in a large variety of Journals. Several extensive bibliographies have been prepared by the Environmental Sciences Branch, DBM, AEC. However, most of the work to 1967 is summarized in these symposium volumes.

Radioecology, edited by Vincent Schultz and Alfred W. Klement, Proceedings of the First National Symposium on Radioecology, Colorado State University, 1961. Reinhold Publishing Corp., 1963.

Ecological Effects of Nuclear War, edited by G. M. Woodwell, Brookhaven National Laboratory, Publication 917 (C–43) 1965.

Radiation and Terrestrial Ecosystems, edited by F. P. Hungate, Proceedings of the Hanford Symposium on Radiation and Terrestrial Ecosystems, 1965. Pergamon Press Ltd. 1966.

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*In 1965 a symposium on *Radiation and terrestrial ecosystems* was organized by the Atomic Energy Commission and Batelle Memorial Institute at the Hanford Laboratory, with 46

Environment of the Cape Thompson Region, Alaska, edited by N. J. Wilimovsky and John N. Wolfe, PNE–481, U.S. Department of Commerce, 1966.

Symposium on Radioecology, edited by D. J. Nelson and F. C. Evans, Proceedings of the Second National Symposium, Ann Arbor, Michigan, 1967. CONF-670503, U.S. Department of Commerce, 1969.

DIRECT ECOLOGICAL EFFECTS OF FALLOUT FROM UNDERGROUND NUCLEAR EVENTS

In this section, I am focusing on radiation effects ecology as it relates to underground nuclear testing, this being one of the specific areas of my own research.

Most of the data and concepts on the irradiation of ecosystems have come from the use of experimental radiation fields using a point source of gamma radiation usually Cesium^{137} or Cobalt^{60}. Major programs have been developed at Brookhaven National Laboratory, Oak Ridge National Laboratory, Puerto Rico Nuclear Center, UCLA Laboratory of Radiation Biology, Savannah River AEC Laboratory, Emory University, University of Georgia, University of Tennessee, and the University of North Carolina.

I would like to add as an appendix to this statement a paper of mine on *Ionizing Radiation and Homeostasis of Ecosystems* which presents a synthesis of many of the concepts which have been developed. Much of the work in this paper is based on studies of a radiation field of several hundred acres created within a 10,000 acre reservation by an air shielded nuclear reactor operated by Lockheed Aircraft Corporation for the U.S. Air Force. This is the only instance in which an ecosystem covering several hundred acres has been irradiated at exposures ranging from above lethal to background levels with the doses approximate those expected from fallout.

The intensive fallout radiation fields produced by the firing of nuclear devices are well recognized. These have been investigated both on the continental United States and in the Pacific Test Area by scientists interested in possible effects on the environment. However, the anticipated radiation effects were obscured by the effects of heat, blast and dust, so that it was impossible to obtain data on radiation effects as such.

Three recent cratering events of the Plowshare series at the Nevada Test Site, Palanquin, Cabriolet and Schooner, have provided small fallout fields, in which the effects from the resultant radiation are unequivocal. In 1966, Dr. W. A. Rhoads and I, along with a team of scientists and in cooperation with the Atomic Energy Commission, initiated a series of studies on these events. They began with Palanquin which had occurred in April of 1965, and continued with Cabriolet which occurred in January of 1967, and Schooner which occurred in December of 1968. A much more comprehensive program is now being planned for Sturtevant, scheduled to occur in the next few months. Further expansion of these studies is anticipated.

Our investigations on Palanquin and Cabriolet have been completed, and the findings presented below are based on these two events. These studies have resulted in (1) the first unequivocal report of direct damage to vegetation from fallout radiation, (2) the first documentation in the field of the beta doses from fallout radiation, (3) the first documentation that the beta component of fallout is the primary cause of plant damage, (4) the first account of comparative radiation effects from fallout radiation and gamma radiation from a point source in the same ecosystem, (5) the identification of a shrub, *Grayia spinosa*, with an LD_{100} radiation sensitivity well above 100,000 R, which is much higher than that previously recorded for a woody plant and (6) the first report of a comparison of predicted and measured dose effects from seven kinds of data.

Palanquin and Cabriolet were of approximately the same size, each less than 5 kilotons. Although occurring at different times, they were less than one-half mile apart in the far northwest corner of the Nevada Test Site on Pahute Mesa, southwestern Nevada, in Nye County. This area is in the southern end of the Great Basin Desert and is gently rolling with an average elevation of 2000 meters. The vegetation is dominated by two species of sage brush, *Artemisia arbuscula* Nutt. subsp. nova (A. Nels.) Ward, and *A. tridentata* Nutt., *Juniperus osteosperma* (Torr.) Little, *Grayia spinosa* (Hood) *Moq.*, two species of Mormon Tea, *Ephedra viridis* Cov. and *E. nevadensis* Wats., and several grasses.

These two craters, along with the extent of observable effects, are shown in the photomosaic, figure 1. Since Palanquin had occurred more than a year pre-

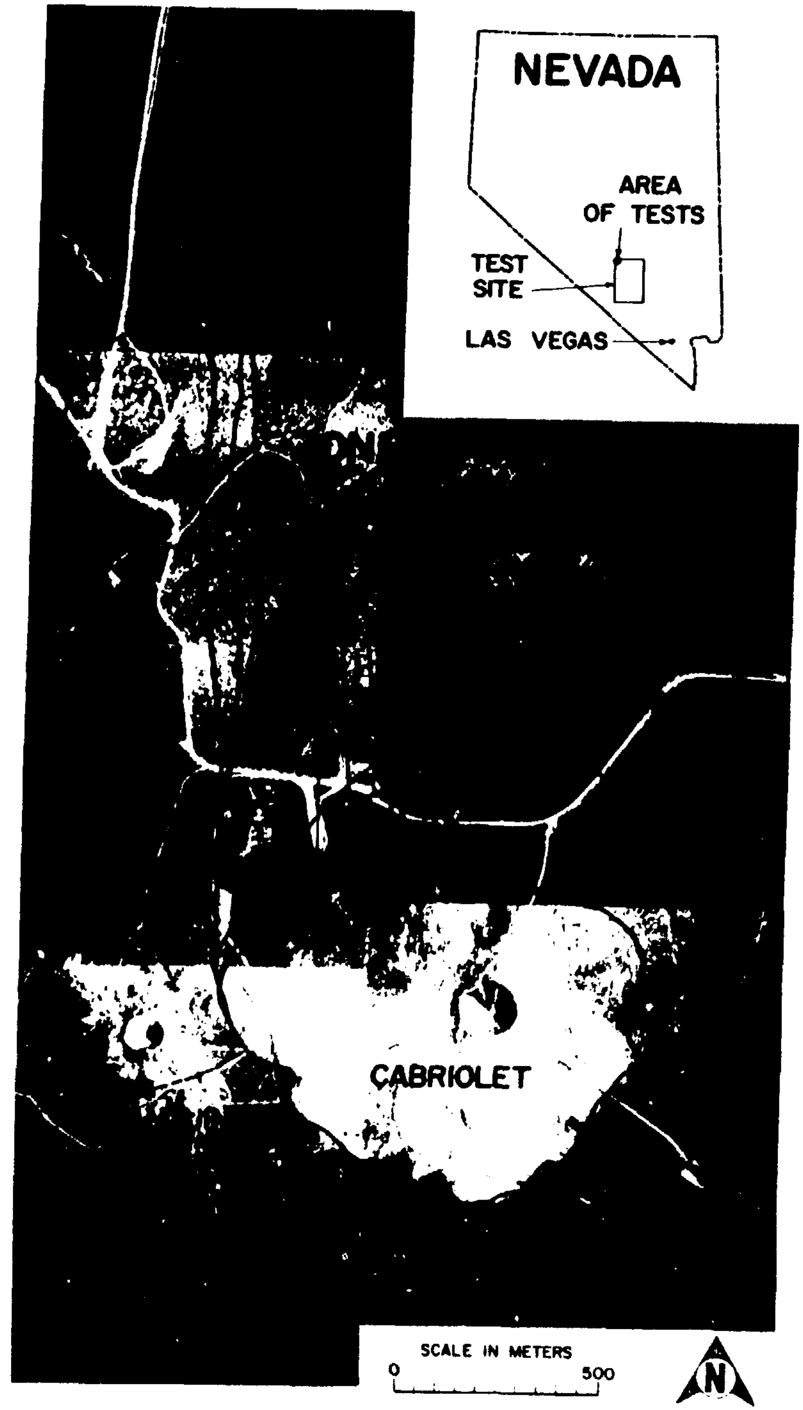

FIGURE 1.—Craters and zones of damage for two Plowshare events at the Nevada

viously, it was necessary to attempt to extrapolate the total dose from the existing dose rates. A close correlation was found between the existing gamma radiation levels and the patterns of damaged vegetation. Observations on the vegetation were made primarily in the transition zones from where all plants of a given species were dead to where all were living. Although both dead and damaged plants of several species frequently occurred together, the dose rates associated with certain effects did vary among them. This suggested a difference in radiation sensitivity among the species, if the general assumption is accepted that in a fallout field the total accumulated gamma doses are directly proportional to the dose rates. Estimates of the total gamma dose for this event were based on fallout having arrived 4 to 5 minutes after detonation, a time period determined by the distance from ground zero and wind speeds, and an assumed radioactivity decay with time at an exponential power of −1.2, ($t^{-1.2}$). Dose estimates derived in this manner are subject to error from several factors such as the possibility of redistribution of radioactive materials at some time subsequent to the initial deposition, the inherent error in reading field instruments at the low dose rates encountered and the possibility of some variance from the assumed decay rate. However, doses calculated from dose rates which had been taken within a few days following the detonation, were in general agreement with those reported here.

Because of the characteristics of local fallout patterns, combined with the high frequency and density of the two species of *Artemisia*, a zoned map could be drawn which summed up the conditions of these species downwind from the crater. A similar pattern for Cabriolet was subsequently established. Referring to the photomosaic, Zone 1 is an area where all the *Artemisia* shrubs were killed. In Zone 2 they were severely damaged or killed, and in Zone 3 were slightly damaged or appeared normal.

To determine experimentally the gamma doses required to produce comparable effects under the same environmental conditions, a 1000 Ci ^{60}CO source was set up about 1 mile south of the Palanquin ground zero on a similar site. The source remained unshielded for 34 days in order to irradiate the surrounding vegetation. Gamma doses ranged from 100,000 R near the source to 100 R at 100 meters. This experiment indicated that the gamma doses observed over much of the Palanquin pattern were far too low to account for the observed damage from the 34 day experimental exposure, even when allowing for the increased effectiveness of radiation doses from rapidly decaying radionuclides.

To account for this discrepancy, the hypothesis was adopted that the beta radiation component of the fallout field was primarily the cause of damage. Studies of the effects of beta radiation as a potential cause of plant damage has not been investigated previously in fallout fields. However, the hazard of beta radiation in fallout to skin has been recognized since the first nuclear detonation at Alamogordo.

The anticipated Cabriolet event provided an opportunity for testing certain aspects of this hypothesis by the measurement of beta and gamma doses in the fallout pattern for correlation with evidence of damage. To obtain such dose measurements, 45 dosimetry stations were established on an arc 610 meters from the proposed Cabriolet ground zero with distances of 15 to 27 meters between them. Each station contained an array of dosimeters designed to differentiate between beta and gamma doses. A new series of special dosimeters were constructed especially for this project. They were distributed on and at several heights above the soil surface and on the fronts, tops and backs of shrubs, and also on plant phantoms. Figure 2 presents the gamma and beta doses to *Artemisia* as indicated by those dosimeters placed on the shrubs. This figure clearly shows that the beta doses were very much greater than the gamma, and there was also a difference between the doses to the fronts and backs of the shrubs.

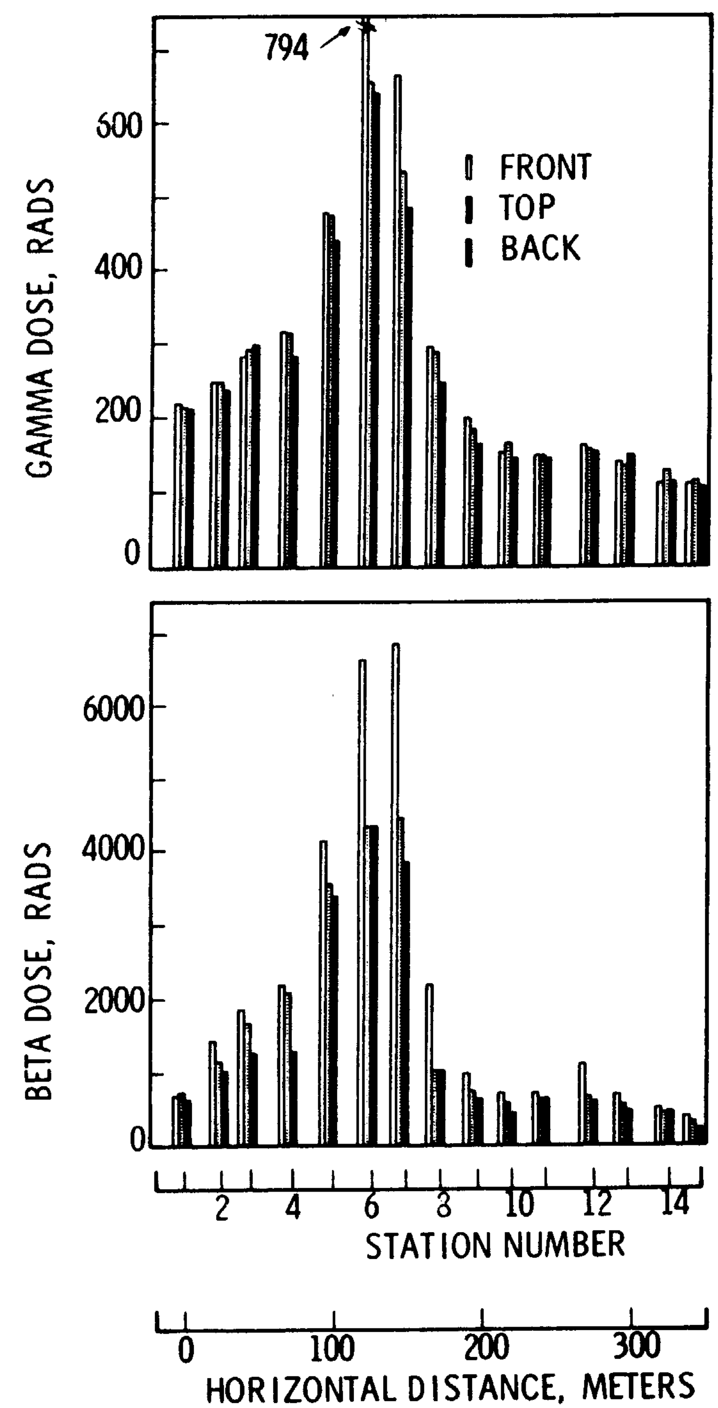

FIGURE 2.—Beta and gamma doses from fallout at Cabriolet along an arc of dosim-

The ratio between beta and gamma doses ranged from 12.5 Rads beta to 1 Rad gamma at the center of the fallout pattern to not less than 4:1 at other locations.

The event occurred in January when the plants were dormant. Hence, no observable effects were anticipated or observed until spring growth was underway. The first observable changes occurred in late May, 119 days post-detonation. The most sensitive of the irradiated plants, the two species of *Artemisia*, showed a slight yellowing and an absence of inflorescence development along the dosimetry are where doses were the highest. By late summer, those receiving less than 300 Rads appeared normal, while a damage gradient occurred inbetween. Junipers receiving approximately 5000 Rads required 18 months for death to occur. Desert shrubs which were close in to the crater and therefore received several times these doses, died within a few weeks.

A detailed account of the data obtained is not justified for this presentation. Rather, I would like to summarize the principal findings, and then refer to two preliminary publications which describe these observations in much greater detail. Other papers, now in preparation, will bring the work up to date.

Table 1 compares the predicted and observed sensitivities of four shrubs, based on several kinds of criteria. The best single criteria for the prediction of radiation sensitivity of plants is that developed by Arnold Sparrow and his colleagues. They have demonstrated a relationship between radiation sensitivity and cell nuclear characteristics. In the Table, Item 1 gives the nuclear volume; Item 2, the interphase chromosome volume; Item 3, the LD_{100} predicted from the chromosome volume based on a 16 hour gamma irradiation; and Item 4 adjusts this prediction to that expected from simulated fallout radiation.

The last three items show the observed LD_{100} radiation sensitivities. Item 5 is based on the 34 day gamma irradiation experiment; Item 6 is based on estimates from post-event dose rates at Palanquin; and Item 7 shows the observed LD_{100}'s in the fallout field at Cabriolet.

TABLE 1.—PREDICTED AND MEASURED LD_{100}'S FOR 4 GREAT BASIN DESERT DOMINANTS BASED ON NUCLEAR CHARACTERISTICS, POINT GAMMA SOURCE IRRADIATION, AND 2 FALLOUT RADIATION FIELDS

	Artemesia (2 species)	Juniperus osteosperma	Grayia spinosa
1. Nuclear volume (μ^3)	477	517	128
2. Interphase chromosome volume (ICV) (μ^3)	26.5 or 13.2	23.5	4.0
3. LD_{100} predicted from ICV; 16-hour γ-irradiation (R) 2d year	1,700 or 2,700	1,800	7,000
4. LD_{100} predicted from ICV; simulated fallout decay (R)	900 or 1,350	900	3,500
5. LD_{100} measured by 34 day γ irradiation field experiment (R) 2d year, September 1966	30,000	6,000	>100,000
6. LD_{100} *estimated from 14-month post-event dose rates, palanquin* fallout field (R) (γ dose) April, 1965	1,062	1,062	2,654
Beta component (not estimatable)			
Total dose (not known)			
7. LD_{100} measured in cabriolet fallout field (rads) January 1967:			
Gamma component	543	543	
Beta component	5,000	5,000	
Total dose	5,543	5,543	[1] 5-10,000

[1] Estimated dose.

The most striking thing about this Table is that the gamma doses from the 34 day exposure (Item 5) required for 100% lethality are much higher than those predicted (Items 1, 2, 3, & 4) or those measured in the fallout fields (Items 6 & 7). Part of this difference can be accounted for by the dose rate, and in some instances by the physiological state of the plants at the time of irradiation. With regard to dose, 75% of that from fallout comes within the first 12 hours and this high rate has been determined to be twice as effective as a 16 day exposure, and even more effective than for a 34 day exposure. Using *Artemisia* as an example, if the experimental gamma dose was adjusted to a fallout dose rate by a reduction of one-half or even one-third, the LD_{100} would be 15,000 R or 10,000 R respectively. This is still several times higher than the 1350 R predicted in Item 4. For this example, any adjustment for physiological state would provide an even greater differential.

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Of even more interest is that for the Cabriolet fallout field. The gamma dose required to provide 100% lethality was only 543 Rads, while the beta component was 5000 Rads. Thus it appears that the beta component of the fallout radiation is the effective radiation, and also that it may have a higher relative biological efficiency than does gamma, at least under the circumstances tested.

SUMMARY

These and other data, including that obtained so far from Schooner, may be summarized and evaluated as follows. This considers only the direct effects from the fallout. The subsequent distribution and fate of the radionuclides is excluded.

1. In these events, the fallout materials which released sufficient radiation to produce observable effects were contained within a relatively small and well defined area, with a maximum distance of about 3 miles downwind from the crater for Palanquin (1965), less than one mile for Cabriolet (1967), and for the much larger shot, Schooner (1968), less than 2 miles.
2. The close-in fallout distribution was determined by the nature of the event, the substrate and by weather conditions, especially wind.
3. Dose rates fall off sharply with distance, and the beta-gamma ratios vary across the pattern.
4. The microdistribution of fallout particles varies greatly with the topography, the ground surface, and the structure of the vegetation and of individual shrubs. The front sides of shrubs intercept more particles than the backsides, and hairy leaf surfaces and closely compacted leaves collect more than smooth or widely spaced surfaces. For example, *Artemisia*, with compact hairy leaves surrounding the bud, will catch many more particles than would the smooth surface of the Juniper twigs, and therefore, receive a much greater dose.
5. Weather conditions subsequent to the event are significant in redistribution of the particles. Rain during or immediately following the event would wash the particles from the vegetation and vary their concentration on the surface, thus affecting the dose received.
6. The *extent of damage is also affected by the physiological state*, i.e. dormant or active growth. This may result in delayed effects, for irradiation occurring during the dormant season at around or less than lethal doses will not be evident until the growing season.
7. The beta radiation field from the ground surface extends upward for only 2 or 3 meters while the gamma field is, of course, far more extensive. Thus, as the distance above the ground increases, the capacity of the plant to intercept the fallout becomes increasingly important in terms of dose received.
8. Because of the above factors, considerable variation should be expected among various kinds of ecosystems, i.e., tundra, coniferous forest, or tropical forest.
9. The beta dose from the fallout of these events is 4 to 12 times higher than the gamma and as such becomes the critical factor. This is the first time the beta component has been measured. All previous estimates of damage from fallout have been based on the gamma component alone, as for example predictions on the ecological effects of nuclear war. These data suggest that a re-evaluation be made with respect to organisms that are not adequately shielded as are mammals by their thick skin. Plants with their growing tissues protected only by an epidermis or by thin scale leaves, as well as many invertebrates which also have little protection and all exposed microorganisms should be included in such a reevaluation.
10. The counterpart to effects is recovery. This may take years, decades, or centuries depending upon the nature of the damage, the kind of ecosystems, the ecological nature of the survivors, the availability of repopulating units such as seeds, spores, underground perennating organs, and immigration, the climate, and the assistance of man. Radiation, like other catastropic stresses, such as fire or hurricane, tends to throw an ecosystem back to an early stage of succession. It is too early yet to understand the nature of the recovery of these desert ecosystems. At Palanquin, however, within 3 years the radiation resistant grasses have now replaced the dead *Artemisia* shrubs.

Radiosensitivity of certain perennial shrub species based on a study of the nuclear excavation experiment, Palanquin, with other observations of effects on the vegetation. CEX–68.4. Civil Effects Test Operations, U.S. Atomic Energy Commission, May, 1969. 33 pp. (with W. A. Rhoads and R. A. Harvey).

Ecological and environmental effects from local fallout from Cabriolet 1. Radiation doses and short-term effects on the vegetation from close-in fallout. PNE–956. Plowshare-Civil Industrial and Scientific Uses for Nuclear Explosives. June 25, 1969 (with W. A. Rhoads).

COMMENTS AND RECOMMENDATIONS

1. The advantages as well as the disadvantages of underground nuclear events should be considered jointly as well as unilaterally, the ultimate test being their contribution to human welfare.

2. Technology should work hand-in-hand with society, and not be its master. It may be necessary to drastically alter or even withhold certain technological applications, if the short and/or long range consequences are adverse to man. Likewise, man must be prepared to pay the increased costs of technology if necessary, in order that it serve his best interests. The majesty of technology is not sufficient unto itself.

3. The above is really a plea that technology and ecology form a working union, with respect to the environment. This is barely beginning to happen at the Nevada Test Site. Engineers, physicists and ecologists have begun to communicate with each other in the last 2 years. To my knowledge, this is the first time that even a minimum of basic ecological planning has gone into the program of a nuclear event, and such plans are expanding. This statement excludes, of course, the extensive inputs on human health and safety with respect to heat, blast and fallout and the intensive efforts of technology to control and reduce the levels of radioactive effluents.

4. With respect to the above, our knowledge of environmental effects of underground nuclear events now lags far behind that of the technology required to produce them. An intensive and even crash program is required to bring them into balance.

5. The environmental effects can be subtle and long lasting as well as immediate and obvious and will inevitably infringe on man.

6. With respect to radiation ecology, a relatively small but very able group of scientists, around 300, are actively working in the field. Their agency associations have been primarily with AEC and to a lesser extent with USPHS. Their investigations are and have been basic and objective. Therefore, in my opinion the proposed study commission, if established, should work closely with the AEC for radiation effects and the AEC and USPHS for work with radioisotopes.

7. However, radiation ecology is closely integrated with other aspects of ecology as well as with other applications of nuclear energy such as nuclear power reactors. For example, much of the work of the International Biological Program, particularly on ecosystems, and much of the basic research funded by NSF is pertinent to this area.

8. The proposed study will provide a means of informing the public on various aspects of nuclear energy. A substantial responsibility goes with this, for the general public is not only poorly informed but often misinformed and their reaction to many aspects of nuclear energy are more emotional than rational. I personally join with many others in believing that one of the most significant challenges facing those involved in nuclear energy is to provide the public with thoughtful and constructive information regarding both its advantages and disadvantages.

9. This last recommendation is concernd with the kinds of information needed on radiation effects. Areas of particular concern include: (1) sublethal effects from radiation doses of less than 2–300 Rads. (2) effects of beta radiation to complement the extensive work done with gamma radiation. (3) subsurface effects and (4) effects extended in much greater depth in physiological processes of organisms as well as on the dynamics of ecosystems. These studies contribute directly to man by providing a better understanding of basic radiation effects on his environment, as for example productivity and stability.

Appendix

Ionizing Radiation and Homeostasis of Ecosystems

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This paper is a review of the concept of homeostasis as applied to ecosystems. It emphasizes the fundamental distinctions among various methods for the study of radiation effects on homeostatic mechanisms within ecosystems, presents one line of evidence for evaluation of these effects, and reviews the fundamental bases necessary for predicting effects of radiation stress on homeostatic mechanisms in other kinds of ecosystems. The paper is restricted to radiation effects *per se* with particular reference to nuclear war, and does not pertain to the distribution and fate of radionuclides in the natural environment.

A historical perspective is essential to understanding current concepts of homeostasis within irradiated ecosystems. Although most studies in the early days of the Atomic Age were medically or economically oriented, the significance of ionizing radiation as an ecological factor in man's natural environment did attract the interest of many biologists. Some of the first research programs were initiated in the 1940's at the Pacific Atoll test site,[20] and later at the Nevada test site.[16] The first major step in the development of radiation ecology, however, was the establishment in the mid-1950's of an Environmental Sciences Branch within the Division of Biology and Medicine, U.S. Atomic Energy Commission, to study direct effects of radiation and the fate of radionuclides in man's natural environment.

Among the first papers to attract widespread scientific and political interest was one of Wolfe's[18] on the ecological effects of nuclear war. Mitchell[8] presented perhaps the first Civil Defense analysis of ecological problems relating to postnuclear war recuperation. Studies were begun in 1956 at Emory University on radiation effects on ecosystems using primarily short-term exposures from point sources,[15] and in 1960 at Brookhaven National Laboratory using primarily continuous irradiation from point sources.[19] Many other programs, too numerous to review here, have been initiated in recent years. The First National Symposium on Radioecology was held in September 1961 under the auspices of the Environmental Sciences Branch of the USAEC and the American Institute of Biological Sciences.

This brief review shows that most of our concepts have been developed within the past ten years and that most publications on radiation ecology have appeared within the last five years. Today research on radiation effects on ecosystems, which involves homeostasis, is in a vigorous and actively expanding condition. With the broad outlines developed, the trend is toward research in greater depth, with increasing emphasis on physiological as well as ecosystem ecology.

HOMEOSTATIC MECHANISMS

The term homeostasis is used to emphasize the concept that ecosystems have regulatory mechanisms paralleling those of organisms, and that ecosystems react to radiation stress in the same manner as to other environmental stresses. Utilizing these regulatory mechanisms, they adjust to continuously changing environmental conditions, including diurnal and other cycles, and react and adjust to various catastrophes.

Physical mechanisms affecting homeostasis include conditions of the physical environment such as temperature, moisture, light and ionizing radiation, nonliving materials, and energy flow. Structure refers to the spatial relationships of the various species, such as the trees and shrubs which form a closed canopy in deciduous forests, or the widely spaced plants of the desert, which form an open or interrupted canopy.

Species composition includes the diversity of species, the abundance and distribution of the component species, and the function of these species within the community in performing autotrophic or heterotrophic activities. In general, the greater the diversity, the greater the resources of the ecosystem in adjusting to stress. If certain species are removed by insect injury, extreme drought, ionizing radiation, or other stresses, the availability of replacement species becomes significant. Most ecosystems contain an ample supply of replacement organisms in the form of seeds and underground perennating organs. The removal or alteration of the overstory or other parts of the community changes conditions and presents opportunities for replacement organisms to establish themselves. These immediately available replacement organisms could have been left by prior successional stages or carried in by wind, birds, and other means. When an eastern deciduous forest is cut over, weed seeds that have been carried in by wind and other sources can germinate because of the changing conditions and within a matter of weeks and months re-establish a ground cover.

Tolerances to physical conditions, including ionizing radiation, temperature, moisture, and light, provide other homeostatic mechanisms unique to each species.

Still another ecosystem characteristic is productivity, which refers to the amount of energy fixed per unit of time. Earlier stages of succession are generally thought to be more productive than later stages because of the rapid change in species composition with time.

One of the most difficult homeostatic mechanisms to evaluate involves the biological interactions within and among populations. These include, for example, growth rate, growth form, age distribution, physiological state, competition, predation, and parasitism. Such interactions can vary widely and are constantly compensating so that over a long period of time there is a leveling off of species activities, even though fluctuations are essential to maintain the flow of energy, ample food supply, and other conditions within the ecosystem.

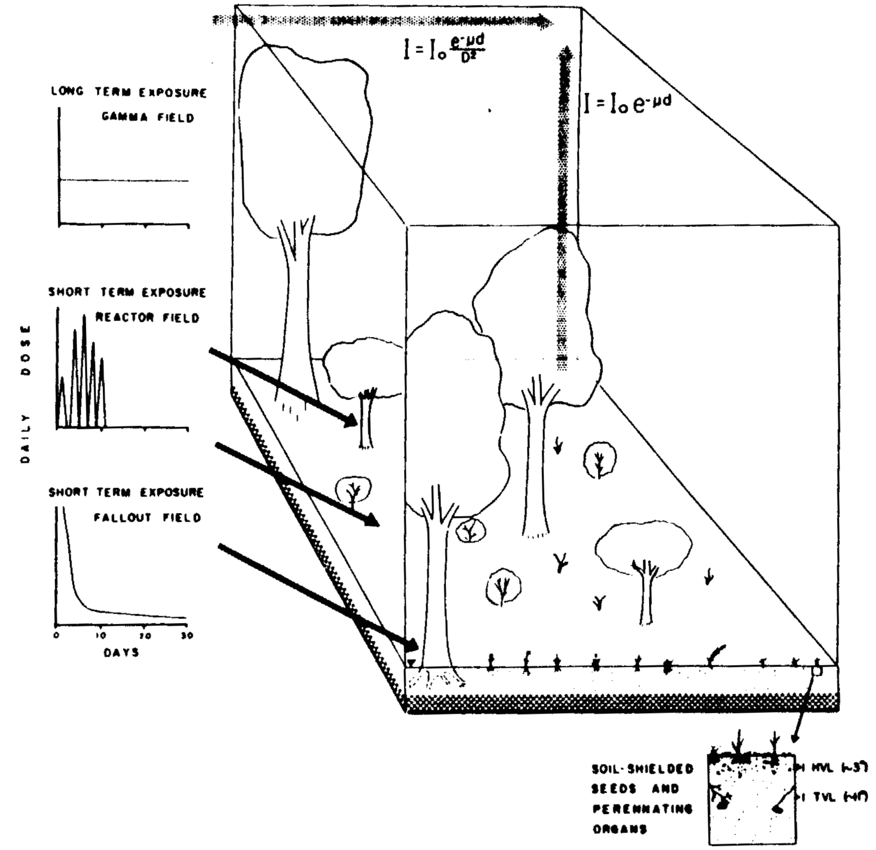
The important point in this brief discussion of homeostatic mechanisms is that ionizing radiation is an environmental stress on organisms and ecosystems, and as such must be considered as another environmental factor. This concept has been developed by Platt et al.,[12,13,15] Woodwell,[17,19] and others.

METHODS FOR IRRADIATING ECOSYSTEMS

Four basic methods, with many intermediate combinations, may be used for irradiating ecosystems. A three-dimensional model of an ecosystem, presented in terms of duration, dose rate, total dose, and dose distribution, is used in discussing these methods (Figure 1).

The methods are (1) short-term exposure from a uniform radiation field, followed by recovery; (2) short-term exposure from a point source, followed by recovery; (3) long-term or chronic exposure from a point source with concomitant adjustment to the continued stress; and (4) direct radiation from a nuclear explosion, accompanied by heat and blast, followed by recovery. In this paper, *acute* means exposure up to several hours; *short term*, exposure from several hours to several weeks;

Figure 1. A comparison of the radiation distribution from a point source and a fallout field. This three-dimensional model of an ecosystem demonstrates dose characteristics for two sources of radiation, one a uniform blanket of fallout, and the other a point source located away from the area studied. The lower right insert demonstrates environmental shielding of soil organisms, ½ of the radiation being attenuated by about 3 in. of soil, and 9/10ths by about 11 in. The inserts on the left show 3 kinds of dose rates over a 30-day period.



long term or *chronic*, exposures of several months or more; *low-level* dose refers to doses in tens and hundreds of rads, and *high-level dose* to doses of tens of thousands of rads.

Short-Term Exposures From Uniform Radiation Fields

Fallout received as a uniform blanket on the surface of the ground would provide a "short-term exposure" (Miller, this symposium). The inverse-square law does not apply to such a uniform radiation field, and radiation dispersion is a function of the coefficient of absorption of the medium through which the radiation travels. Upward distribution of exposure is limited only by absorption by air and any vegetation that may be present. Distribution downward would be limited by absorption by the leaf litter and soil. Since the attenuation of upward distribution is relatively slight, dose distribution above ground from gamma irradiation is relatively uniform. Attenuation downward through the soil would be abrupt, however, the half-value layer for gamma radiation through soil being $\approx$3 in., and the 1/10 value layer $\approx$11 in.

The distribution of beta radiation would be negligible, because of its high attenuation by air as well as soil. In the event of interception of fallout particles by leaves and twigs, the beta activity could be of some significance, because of its proximity to sensitive tissues in meristems.

Exposure rates and period of exposure are of particular significance. As shown on the left side of the diagram in Figure 1, the decay of fallout radioactivity is very rapid, two-thirds of the dose to infinity coming in two weeks. The exposure from fallout, then, is a short-term exposure by the definition used here. Recovery of the ecosystem would begin as the stress was relieved. None of our data suggests that the continued low-level radiation received from the decay of fallout particles after the first two weeks would have any significant effects on an ecosystem, although it may have some effect on certain species within the ecosystem. One example of this type of exposure is reported by Conard[2] at Rongelap. No serious effects were found in natural vegetation after exposures from fallout as high as 3000 r.

Short-Term Exposure From a Point Source

In certain experiments it has been convenient to use a single, centrally located source of radiation. Dose distribution in this case follows the inverse-square law, so that the attenuation falls off inversely with the square of the distance; there is also attenuation by the medium through which the radiation passes. If the distance from the radiation source were large in comparison with the distance across the area of interest, the change, according to the inverse-square law, would be relatively small, and dose distribution would approximate that received from a uniform fallout field, as in the case above.

Attenuation by the leaf litter and soil would follow the same general rule, since penetration into the soil is not greatly affected by the angle of incidence at which the radiation arrives. In the case of rough terrain, however, dose distribution below the soil may vary widely. Because of the buildup factor, as well as scattering by vegetation above the surface of the ground, it is possible that in some cases the dose will exceed that received above ground. (This is illustrated in Figure 3, where the dose

distribution over the brow of a hill from the source increases along the surface of the ground relative to the line-of-sight dose because of scatter and buildup.)

If the duration of exposure is limited to a short term of a few hours or days, this system of radiation very closely approximates that received from fallout.

The Lockheed air-shielded reactor with its several hundred acres of irradiated ecosystems is the only large-scale example of this method.

Long-Term or Chronic Exposure From a Point Source

This method is similar to the one immediately above, with one exception. The stress here is continuous rather than temporary, and the effects are correspondingly quite different, since there is no opportunity for recovery as long as the stress is applied. An applicable expression would be "adjustment to the continued stress," with the ultimate consequence being that of a changed situation, with no chance at all of the area's returning to its original condition as long as the stress persists.

An ecological analogue of chronic exposures would be a hot spring, in which the ecosystem by adjustment to the continuously high temperature is quite different from other spring ecosystems. The continued exposure of an oak-hickory forest around a copper smelter at Copper Hill, Tennessee, ultimately resulted, after many years of continued stress, in a denuded landscape, including the top soil. In contrast, forests nearby that are occasionally subjected to intense fires still retain, following such a fire, essential elements of the ecosystem such as soil, and therefore retain the capacity to restore themselves to their original condition within a few years or decades. In another comparison, a deciduous forest climax under 35 in. annual rainfall would probably readjust to a prairie if the rainfall were reduce to 20 in. Likewise, a forest could survive a chronic irradiation stress of 300 r/day, but probably would readjust to a herbaceous ecosystem if the stress were greatly increased.[19] The irradiated forest at Brookhaven is the only example of this method. Radiation, started in November 1961, has been continuous and could be continued until a new equilibrium is reached. Results obtained after a few weeks or months exposure would be short-term data, but of course there would be no recovery.

Direct Radiation From Nuclear Explosions

Effects from this kind of radiation are very difficult to evaluate. The effects from the accompanying blast and heat have been in most instances more severe than that from radiation. Effects from these three stresses often are difficult to separate, and the radiation, coming in a flash, is a mixture of gamma, beta, and neutron doses. Subsequent irradiation of these highly disturbed ecosystems from the radioactive decay of materials in the area apparently produces minor effects in comparison to those above. However, in this case, as in the first two, recovery occurs as the stress is relieved. Atomic test sites provide many examples.[9]

FACILITIES AND EXPERIMENTAL DESIGNS FOR EMORY UNIVERSITY STUDIES

Facilities for the series of studies at Emory University consist of an outdoor gamma irradiation facility on the campus and the 10,000-acre reservation surround-

ing an air-shielded nuclear reactor operated by the Lockheed Aircraft Corporation. The radiation released into the environment around the air-shielded reactor has provided a unique situation. It is the only instance in which an ecosystem covering several hundred acres has been irradiated at exposures ranging from above-lethal to background levels over a period very closely approximating that which would result from fallout. It has the added advantage for our purposes of having received radiation of the order of tens of thousands of rads of accumulated exposure without the effects of heat and blast associated with bomb tests.

The Lockheed 10-MW reactor is in the center of a 10,000-acre reservation in the foothills of northern Georgia, an area of great ecological diversity.[10] The principal vegetation is mixed evergreen-deciduous forest on both moist and dry sites, flood plain forests, and old fields on both upland and flood plain sites.

The irradiated area within which definitive biological studies have been made extends 1000 to 3000 ft from the reactor, depending upon the terrain. The irradiated area is roughly equivalent to that contained within a circle 2000 ft in radius, or almost 300 acres.

This large area has provided a broad numerical base for statistical analyses. For example, the irradiated forest contained an average of 150 trees/acre. Since at least 250 acres of land were wooded, the number of trees >3 in. in diameter in the total experimental sample could be estimated at 37,500. Figures for shrubs and herbs are even more impressive. Sample numbers, therefore, of hundreds and

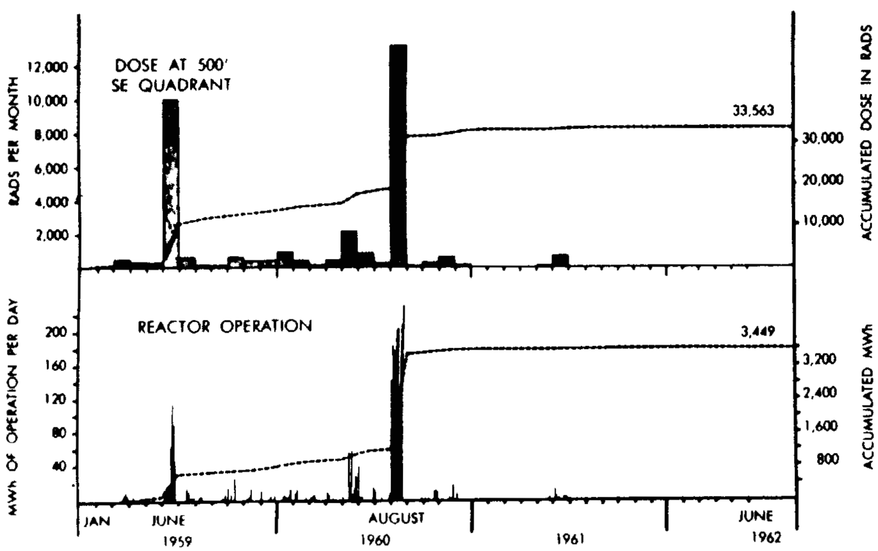


Figure 2. Lockheed reactor operation data beginning with the initial run. The schedule from June 1962 to the present has been approximately the same as that for 1961. Note that >80% of the radiation was released at two times, and that each time it approximated irradiation from fallout in both duration and total dose. At 500 ft, the dose each time was about that of the maximum expected over 2 to 5% of the United States from a 20,000-MT attack.

thousands per experimental plot have made it possible to distinguish between effects of ionizing radiation and those of other adverse environmental factors with which they may easily be confused, such as drought, killing frosts, insect damage, and disease. As these effects also occur in natural ecosystems, such distinctions are of extreme importance.

This large area provided radiation dosage gradients ranging all the way from accumulated doses over several months of 100,000 rads to background levels.

The distances at which biologically effective dosages were received were great enough that the decline in exposure with increasing distance from the source was relatively low. This means that exposure gradients across the crowns of large trees as well as across sizable experimental plots were very slight.

The irradiation pattern followed that of the second method above, short-term exposure followed by recovery.

By good fortune, >80% of the radiation released came in 2 short-term exposures of 2 and 3 weeks respectively, one in June 1959 and the other in August 1960 (Figure 2). The intensity and duration of each of these exposures were by chance similar to those expected from fallout following a nuclear catastrophe. The other 20% was delivered intermittently over a 3-year period at low intensities which had little or no effect at distances >500 ft from the reactor, except on selected species. Thus, the second exposure followed the first by 14 months, and 4 years have elapsed since then. This combination of exposures has provided unparalleled opportunities for observation of recovery over a 14-month period following the first exposure, and over a total of 5 years since radiation was first released.

Two years' study of the area was possible prior to the initial reactor operation, so that experimental plots, control areas, inventories of plants and animals, and other necessary preirradiation procedures could be carried out.

In order to test the observations made around the reactor site, a gamma radiation field was established on the Emory University campus as a control facility for the duplication of critical experiments under controlled conditions.

ECOLOGICAL DOSIMETRY

The expression *ecological dosimetry* is used to emphasize the unique nature of the dosimetry program developed in the rough terrain surrounding the nuclear reactor.[3] To sample adequately the distribution of radiation exposure throughout the 300 acres, over hills and across valleys, at various distances above ground as well as below ground, and at various positions within arboreal vegetation, detection stations were placed at 68 locations, each station consisting of a series of neutron and gamma-ray detectors. Twenty-three were in the reactor "line-of-sight," while the others were arranged to measure various types of shielding by vegetation and terrain.

Neutron activation detectors were used for the field measurements of neutron flux. Thermal and resonance neutron distributions were measured by reactions with cobalt, sodium, manganese, and gold detectors, while the fast neutron distribution was measured by using sulfur, thorium, and nickel threshold detectors. By using a series of these detectors at each field location, a neutron energy spectrum was constructed. The primary gamma-ray measurements were made with film badges used

for the radiation monitoring of personnel, and with chemical dosimeters. These measurements were augmented with a selected number of silver-activated phosphate glass dosimeters. The film badges were used primarily for measuring low doses, while the other two types of dosimeter were useful for high doses. The accuracy of the chemical dosimeters for the dose rates and ranges encountered was within ±10%, while the accuracy of the film and glass dosimeters was within ±20% All dose measurements were expressed in rads (1 rad = 100 ergs/g).

In naturally shielded areas neutrons were attenuated proportionally more than gamma rays by vegetation. Conversely, the soil attenuated gamma rays proportionally more than neutrons. The terrain, however, always produced more total-dose attenuation than did the vegetation. This behavior is in accordance with what could have been anticipated on the basis of the scattering and absorption cross sections of soil and plant atoms for gamma rays and neutrons. In particular, it is believed that the large hydrogen content of plants is responsible for the proportionally large attenuation of neutrons in areas shielded by vegetation.

One of the most significant results of this study was the observation that large dosages result in locations that are partially or completely obscured from the direct beam of the reactor (Figure 3). This can be attributed to initial air scattering and subsequent scattering by terrain and vegetation. Predominant scattering of the radiation in the direction of the incident beams was also observed in shielded areas. Maximum radiation protection in shielded areas was afforded at locations adjacent to the ground level or on the back sides of trees, away from the reactor, and is attributed to absorption by the ground and vegetation of a large part of the scattered radiation which would otherwise have contributed to the dose.

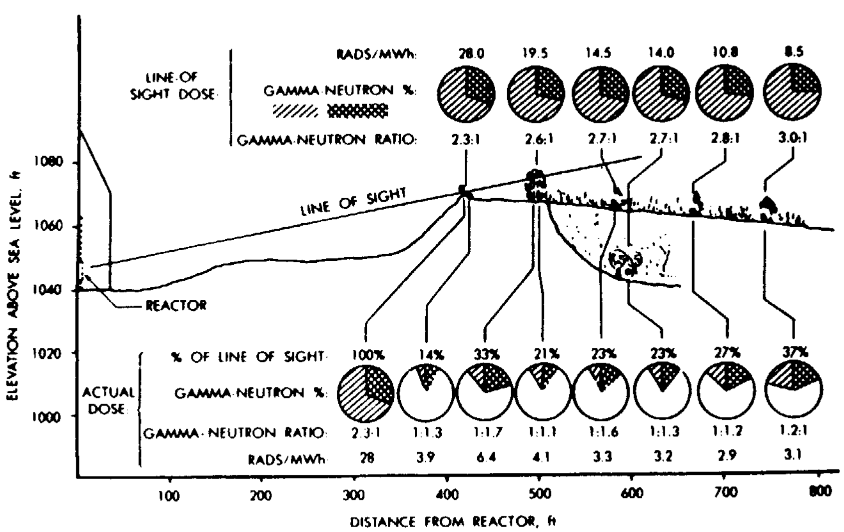


Figure 3. The effects of terrain and vegetation shielding on total dose for a line of stations ESE of the Lockheed reactor, located in a mature oak-hickory stand. The upper circles give the expected line-of-sight dose, while the lower circles give the actual dose, as reduced by terrain and vegetation shielding. Note exaggeration of vertical scale.

From the data of this study, it is possible to reconstruct the radiation history at practically any location in the radiation field and for any given period of reactor operation.

CHRONOLOGY OF EVENTS FOLLOWING IRRADIATION

A hypothetical example based on studies around the Lockheed reactor seems appropriate for this discussion of the potential effects of war. People leaving their fallout shelters in much of the temperate portion of the world in the last part of June 1959, following a nuclear attack from which fallout had delivered as much as 15,000 to 20,000 r of radiation, would be "pleasantly" surprised to find that the familiar surroundings of field and woodland looked as they did before the explosion. The one marked exception would be the areas in which pine trees were evident, for pines receiving 8000 to 10,000 rads or more would have begun to turn brown,

Figure 4. View toward the Lockheed reactor in July 1960, 13 months following the June 1959 exposure. These loblolly pines were dead 8 to 10 months following an exposure of >4000 rads. Removal of the pine overstory released the hardwoods underneath, which accelerated succession.

and in a few days would be a brilliant red-brown all over. If these were scattered through hardwood stands, they would stand out as bright flags in an otherwise apparently unchanged landscape. In fact, with the exception of damage to gymnosperms, there would be little change through the summer until August, when an unusually early leaf fall would be experienced. At least this is what happened in the several hundred acres around the Lockheed reactor. Obviously, these effects would vary as ecological conditions and physiological states within the ecosystem varied.

The relatively high sensitivity of pines (*Pinus taeda* and *P. rigida*) in contrast to the other woody plants was one of the most surprising of all observations, for this was the first time that pines had been irradiated. Within one week after the June irradiation, pine trees receiving doses of 7500 rads or more began to turn a brilliant orange-red and died within a few weeks. Those receiving about 4000 rads took much longer to die. In two years, most of those within 1500 to 2000 ft of the reactor were dead (Figure 4). Discoloration, death of terminal buds, and inhibition of reproduction by seed began with exposures >1000 rads. Apical meristems of pines were much more sensitive than lateral, and at certain doses when apical growth had stopped, radial growth seemed to be accelerated. Photosynthetic rates and tolerance to heat and drought were lowered by exposures of several hundred to several thousand rads.[11]

The second set of obvious effects on these ecosystems occurred in September when oak-hickory stands receiving 12,000 to 15,000 rads had a 7-week-early initiation of litter fall, followed by almost complete inhibition of leaf production the following year.[7] Oaks and hickories receiving 4000 rads had an early leaf fall of only

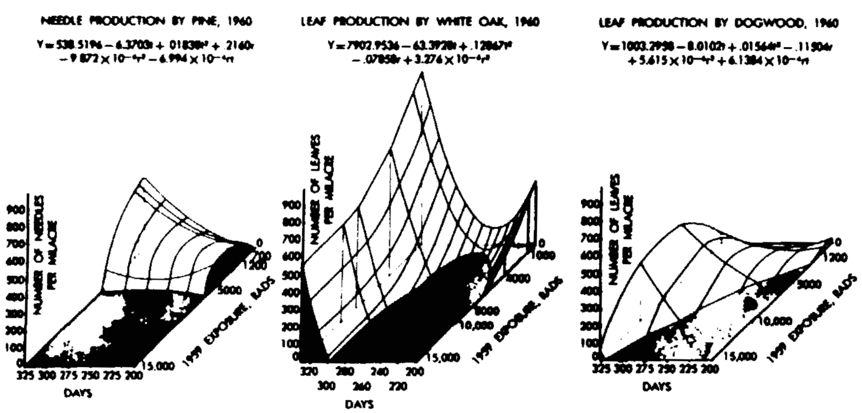


Figure 5. Three-dimensional response surface composed of regression lines for leaf production for three tree species as affected by the June 1959 exposure. Note relationships between time and dose along a transect 500 to1500 ft from the reactor. Reduction of the overhead oak-hickory leaf canopy released the more resistant understory dogwood, so that production for this species increased at the intermediate radiation doses. (From J.T. McGinnis, unpublished data.)

Figure 6. Photograph taken in July 1960, showing the effects of the June 1959 exposure. The white oak on the left received about 12,000 rads and the hickory on the right about 15,000 rads.

one week, as compared to nonirradiated stands, but leaf production was reduced to 39% the following year (Figure 5). Trees receiving 12,000 to 15,000 rads did not go through normal autumnal coloration, having lost their leaves early, while those receiving 4000 rads did go through a normal coloration.

The third set of effects was noted the following spring. In early April, when the hills of northern Georgia were bright with many hues of green due to the half-developed leaves of forest trees, an area up to one mile in diameter around the reactor was still in a state of winter dormancy. The prolongation of dormancy was proportional to the dose received, bud expansion being delayed from 1 to 2 weeks at doses of 10,000 to 15,000 rads. These effects were ascribed directly to the fact that the primordia of the estivating buds were already laid down in June of the preceding summer, but the radiation damage was not apparent until the buds expanded the following spring.

A fourth set of effects also appeared in the spring. On hardwoods receiving 3000 to 4000 rads, almost all the terminal buds were killed, and branches receiving

2 or 3 times this dose were killed back several inches. Almost all of these trees did develop lateral buds, but there was a very pronounced relationship between the severity of the dose and the position on the tree at which lateral buds appeared. Those receiving 3000 to 4000 rads had lateral buds developed on the same twigs on which the terminals had occurred, and, as irradiation increased, the buds developed on larger limbs so that those receiving 15,000 rads had only a few buds along the main trunk (Figure 6). In contrast, the lateral meristems in every case, including

Figure 7. Photograph of a southern red oak branch taken in July 1960, showing typical hardwood response to the June 1959 irradiation. Note aberrant growth.

trees having the highest doses, were bright green and remained so through the spring and summer.

Another obvious manifestation was that almost every leaf produced from a tree on which the terminals had been killed was highly aberrant (Figure 7). Species differences for the most part were slight, but some trees, such as buckeye and sourwood, were markedly different in their responses.

In July of 1960, 13 months following the irradiation, pines that had received >4000 to 5000 rads were dead, and those that had received >2000 rads were markedly affected. Hardwoods receiving 10,000 to 15,000 rads had been devastated (Figure 8) and had only a few flags of leaves, whereas hardwoods receiving 4000 to 5000 rads had leaf production cut to ¼ or ⅓, but nevertheless produced growth adequate for survival. Hardwoods receiving less than this dose were little affected.

Effects on the vegetation of abandoned agricultural fields within the irradiation area may be grouped into three types of response.[4] First, species became arranged along the radiation gradient in successive dominance bands, according to their interacting tolerances for radiation, light, moisture, and other factors. Second, within a uniform radiation zone, elimination of a radiation-sensitive species occurred when that species aborted by tissue breakdown or was unable to complete its

Figure 8. Photograph taken in July 1960, showing effects of the June 1959 irradiation on a mature oak-hickory stand. Doses ranged from 12,000 rads in the part closest to the reactor to about 8000 rads in the foreground. See Figure 12 for another view of this stand 3 years later.

life cycle. The resultant opening in the community was invaded by more radiation-resistant species which were able to complete their life cycles.

Third, perennials often held their position in the community by vegetative growth of perennating organs, although inviable seeds were being produced. Figure 9 shows a normal vegetational pattern for these fields over the first 5 years of succession. The over-all effect of severe irradiation was to throw the field back into an earlier stage of succession. As pointed out above, this was brought about by differential sensitivity of species and the concomitant change in the community structure. Although several experimentally established successional communities received 25,000 to 30,000 rads exposure, being fairly close to the reactor, at no time were these communities denuded. In fact a casual look would reveal no obvious effects, since these changes took place slowly over several months.

The second radiation exposure occurred over a 3-week period in August, 14 months after the first one. The same general kinds of effects occurred. This additional irradiation killed certain pine trees which had been damaged previously. Again, there was an early leaf fall which was roughly comparable to that of the preceding fall, and the following spring there was a comparable prolongation of winter dormancy, with killing of terminal buds and development of laterals.

This series of observations following the second exposure reveals one of the most interesting effects of the entire chronology. The effects of the second exposure were about the same as those of the first, which demonstrated that substantial recovery of

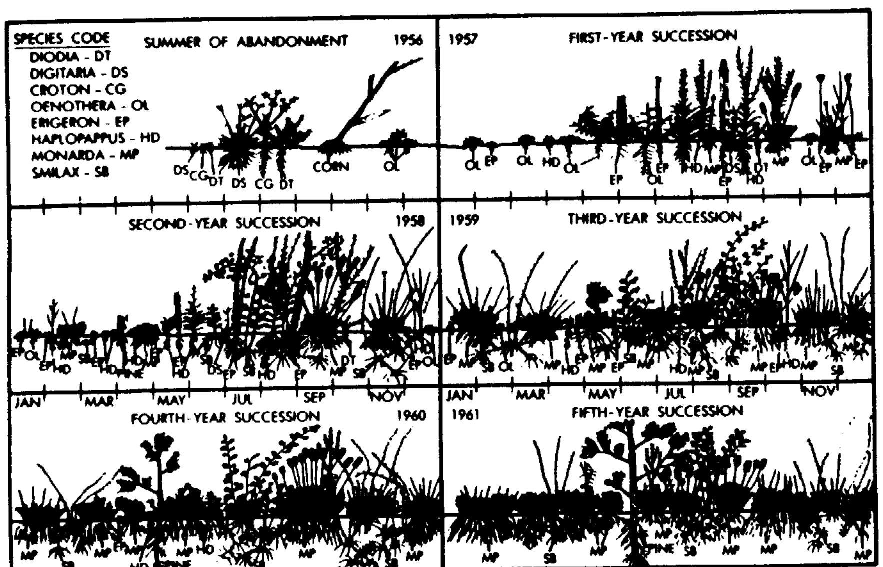


Figure 9. Diagrammatic representation of normal vegetation in the field adjacent to the Lockheed reactor. Radiation (up to 25,000 rads short-term dose) causes changes and reduction in species composition, with a corresponding shift to an earlier stage of succession. (From C.P. Daniel, unpublished data.)

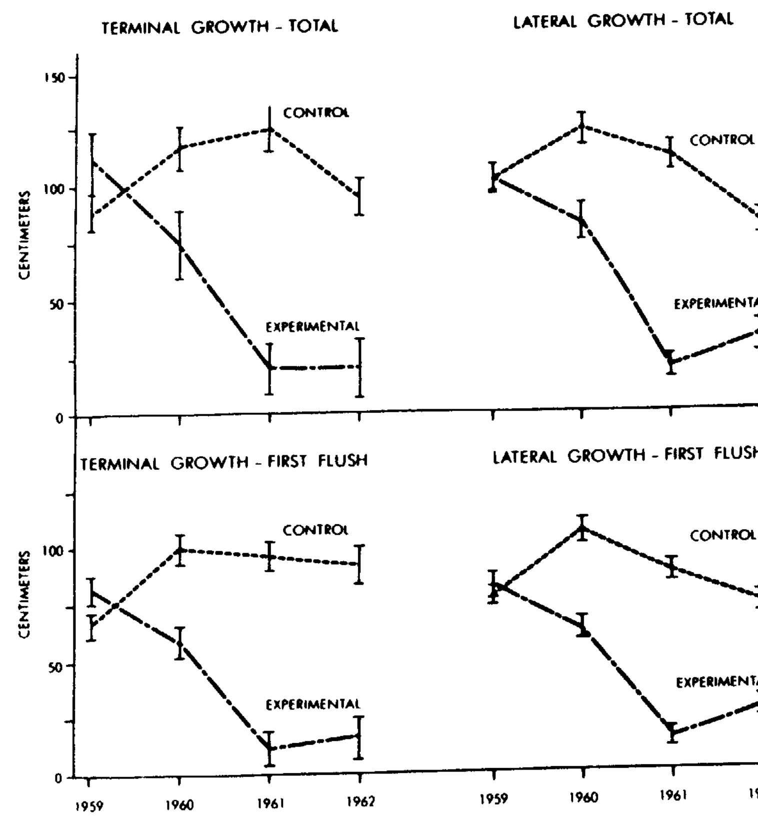


Figure 10. Graphs showing the effects of radiation on a pine stand receiving about 300 rads in June 1959 and 700 in August 1960. There was no visible evidence of damage. The crowns of 20 trees (10 from an experimental stand and 10 from an ecologically comparable control stand) were removed for study in August 1963. Of the 10,000 measurements obtained, those pertaining to four aspects of growth are analyzed. Vertical lines represent twice the standard error. Note that the June 1959 effects did not appear until 1960, and the August 1960 effects until 1961. With no irradiation in 1961, growth was normal in 1962.

damaged plants had occurred. Thus, the effects of the second exposure were not additive, except in those instances in which the trees were already close to dying, and this additional stress killed them, as a bad drought might. The delayed response in tree growth from summer to the following spring was widespread. Figure 10 graphically demonstrates this effect in pines receiving about 300 and 700 rads for the two irradiation periods.

Conditions during the summer following the second exposure were comparable to those of the first, with the exception of those areas in which the overstory had been killed and ground canopy removed (Figure 11). During the first summer this

Figure 11. Infrared aerial view of the Lockheed 10-MW nuclear reactor located in the center of a 10,000-acre reservation. This photograph, taken in June 1961, shows the effects of radiation of the two preceding summers. The dotted line encloses the area of visible radiation effects, 800 to 1500 ft from the reactor.

floor under the opened canopy had demonstrated some sprouting from protected root crowns along with the growth of weeds whose seeds had probably lain dormant for many years on the forest floor pending a time at which conditions would favor germination and growth.

The weed flora was greatly increased the second summer because of the additional seed source from the first summer; at some points the weeds were 8 or 10 ft tall and almost too dense to walk through. The condition at that time seemed to indicate that these forests had been thrown back into an old-field stage and that normal old-field succession would now follow. However, in the third summer, root sprouts which had shown rather poor growth during the first two summers now began to develop rapidly and shaded out the weed flora before it had a chance to get well established. By the end of the third summer, hardwood sprouts began to dominate, and by the end of the fourth summer they had formed a closed canopy under which very few weeds were able to develop. This past summer, the fifth following the first irradiation, there was every indication that these sprouts would continue to develop and that the hardwoods would be restored (Figure 12).

In forested areas in which leaf production was reduced up to 50%, there was no great change in the forest community, with the exception of an increased number of ground-cover plants in the first two summers. Three summers following the first irradiation, the leaf canopy was increasing and leaf aberrations were minor, and by the fourth and fifth summers the trees had returned to a fairly normal appearance.

In the old fields, the areas that had been thrown back to earlier stages of succession moved forward without further interruption, and there was no continued evidence of irradiation damage. The extensive aberrations in leaves and the effects of suppression of reproduction, as in *Smilax*, disappeared and plants were growing normally.[1]

DISCUSSION AND CONCLUSIONS

Information gained over the past 9 years from experimental irradiation of small, manipulatable ecosystems, along with that from 7 years of study at the Lockheed reactor site, is now sufficiently complete to demonstrate some fairly clear-cut cause and effect relationships for short-term exposures, similar to those that would arise from fallout following a nuclear attack.

1. The patterns of effects and recovery in plant populations seem now well established; they are shown graphically in Figure 13. The dose in rads is given in the left-hand column for exposures of 15 to 90 days. The year of development of various

Figure 12. Photograph taken in August 1963 of the same area shown in Figure 8, three years after the second exposure. By this time trees have either died or begun recovery, intermediate stages disappearing. A flourishing growth of root sprouts seems to be returning the area directly to an oak-hickory forest.

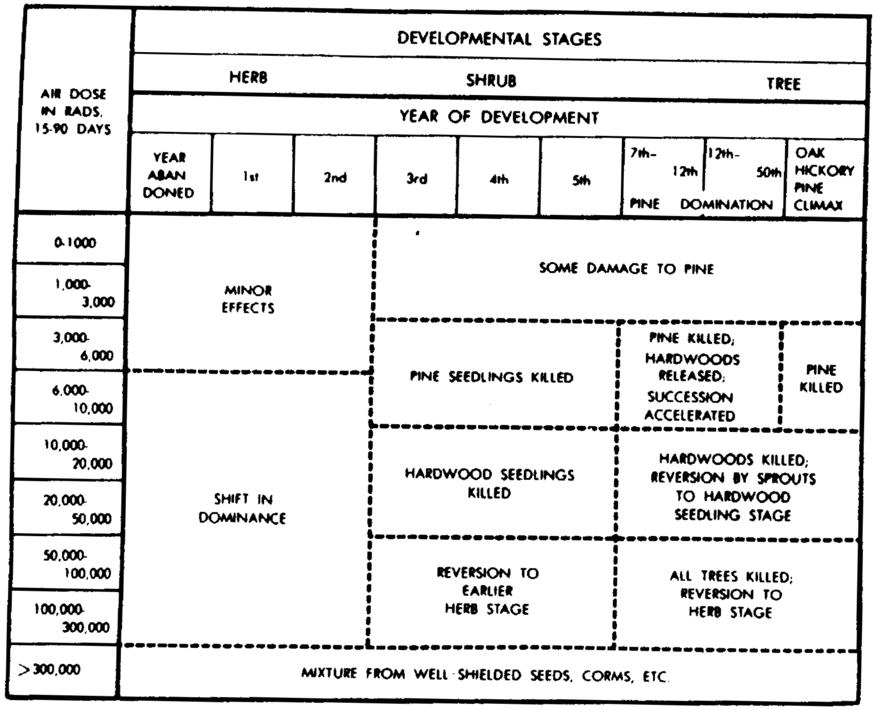


Figure 13. Ecological effects of short-term radiation exposure on temperate ecosystems, based on data from Emory University studies. Doses up to 300,000 rads are plotted against developmental stages from abandoned agricultural fields to climax forests.

ecosystems from the time of abandonment of agricultural fields to mature oak-hickory-pine climax forests is shown across the top. The results indicated would vary somewhat with the severity of other environmental stresses, the time of year of exposure, and other conditions.

Three ecologically significant community types have been studied: herbs, shrubs, and trees (Figure 13). Herb communities can withstand radiation up to >100,000 rads exposure without elimination of the ground cover. Data on effects from the higher doses were obtained in the campus radiation field. For the shrub communities, i.e., the third, fourth, and fifth years of succession, during which the pine seedlings become established, the pine seedlings would be killed by doses >3000 rads and hardwood seedlings by >10,000 rads. If the dose exceeded 25,000 to 50,000 rads, the remaining herb stage would revert to an earlier year of development.

In ecosystems dominated by pine trees, as they would be from the seventh year on, pines would be eliminated first, hardwoods next, and a reversion to a herbaceous stage probably would not occur until 50,000 rads had been received, the latter due

to elimination of root sprouts. The extensive work of McCormick and Platt[5] on granite outcrop ecosystems supports these conclusions.

2. The application of the experimental results to larger geographic regions is difficult, because the irradiated area is an island in the midst of normally developed ecosystems. What would happen if these conditions extended over several hundred square miles, so that most of the areas would have limited access to recolonization by higher forms of life, such as mammals and birds, or by seeds which might be brought in by various agents? A second factor involved in interpretation is that the exposures reported occurred only during the summer. Had the irradiations come at other periods, would the sequence of events have been comparable?

There is a good probability that neither situation would significantly alter the course of recovery as reflected in these studies. The single best argument for this is that recovery of vegetation is not dependent only upon the transport of organisms from other areas. Around the reactor site, replacement of killed or severely damaged plants was accomplished by the growth of seeds and underground perennating organs which were there before irradiation.

With respect to the larger animals, it is reasonable to suppose that extreme injury would be limited to areas of tens or hundreds of miles across, and there would be large numbers of refugia receiving less than lethal doses, from which repopulation of seriously damaged areas could begin. While this might require some time, repopulation could occur.

The invertebrates for the most part have radiation-resistant or environment-shielded stages so that populations would become re-established in the same sense that plants would recolonize the area.

The question of wildly fluctuating populations of insects and other pests which would seriously affect the balance of nature following irradiation has been raised on many occasions. Our observations within these ecosystems suggest that great caution must be used in making such predictions. The effects from a forest fire might well be much more severe than the killing of hardwood trees in a comparable area by ionizing radiation. Yet from such ecological analogues as fires, population fluctuations usually have not been of the kind that would seriously affect man's ability to survive. Every time a tree dies in the forest, or a hurricane causes a severe wind-throw and the canopy is changed, comparable wildly fluctuating populations of the microinsect fauna occur, but the ecosystem compensates for this in many ways.

3. Irradiation in itself does not eliminate ground cover or leaf litter. In fact radiation which would kill the overstory of hardwood forests would probably leave the underground portion of the ecosystem relatively undisturbed because of shielding from the soil, although there would be changes in the microenvironment in terms of light, moisture, wind, and relative humidity.[6]

Unless an accompanying fire removed the ground cover, erosion would not be a factor. In the event that fires swept through the area following irradiation, there would probably be the same kind of recovery that has been observed many times before as a result of fires which have sometimes covered several hundred square miles at one time.

4. Radiation stress, like other stresses, tends to throw the ecosystem back to an earlier stage of development. Furthermore, the time of year of irradiation is of great

importance, and some of the most significant effects may be delayed for many months.

5. Sufficient information is on hand from many sources to make general predictions for the effects of radiation on other ecosystems. There seems to be general agreement that a rough correlation exists between the ecosystem's structural complexity on one hand, and resistance to radiation on the other, sensitivity of individual organisms to radiation being the principal exception.

One probable ranking of ecosystems in increasing order of resistance to radiation stress would be: coniferous forests, rain forests, deciduous forests, grasslands, tundra, and desert. However, for predictions of greater reliability and depth, much more information is needed. Figure 14 gives the kind of information that ideally is required for each of the functionally significant species as well as for the ecosystem. Nuclear characteristics are applicable in determining the probable lethal dose. For lethal or sublethal doses, the effects are those determined in addition by hormone and other metabolic systems, physiological states, and interactions with other environmental factors. The latter involve the functional relationship of the organism to its ecosystem, and this in turn is related to the ecosystem's own characteristics.

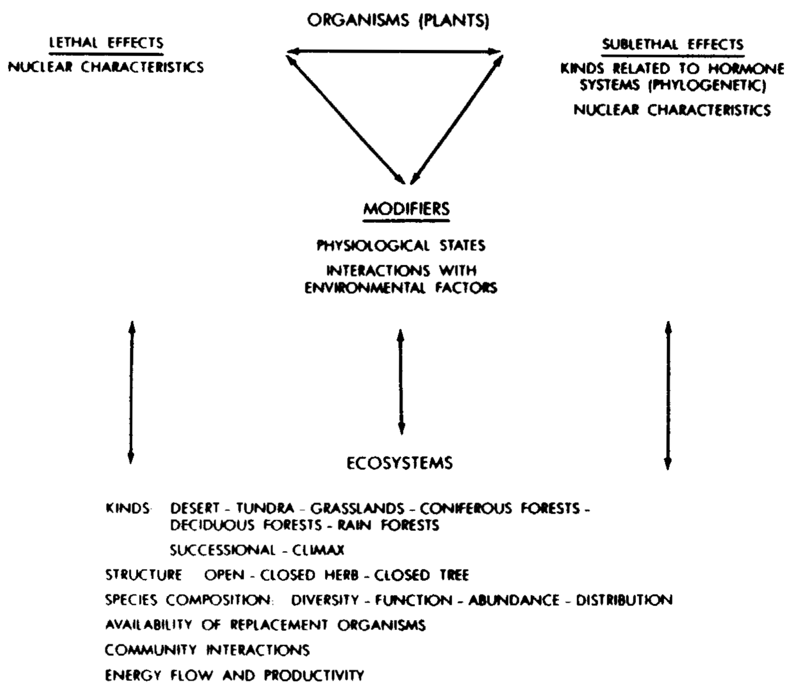


Figure 14. A scheme for homeostatic mechanisms that control radiation stress effects, the upper part relating to organisms (plants) and the lower to ecosystems. Conversely, an evaluation of these factors is revelant to the prediction of radiation effects.

EPILOGUE

Radiation effects and subsequent recovery in ecosystems near the air-shielded Lockheed nuclear reactor constitute the closest approximation of short-term radiation effects without heat and blast on vegetation. The two periods of high-level irradiation, 14 months apart, closely resembled fallout exposures, both in intensity and in duration. The course of recovery apparently has been well established in the five years since the first exposure and the four years since the second. Thus, it has been possible to establish ecological effects on vegetation for doses up to 300,000 rads, plotted against developmental stages from abandoned agricultural fields to climax forests. Since the pine-dominated stage is highly sensitive to radiation, the hardwood stage intermediately sensitive, and the herbaceous stage among the least sensitive, results from these three developmental stages have wide applicability to similar areas throughout the world.

In the event of a 20,000-MT attack on the United States with 100% fission, it has been estimated that 2 to 5% of the country would receive 15,000 r or more within 2 weeks, and 10% would receive 5000 to 10,000 r. The remaining 85 to 88% would receive $<$5000 r, the greatest percentage on the order of 1000 to 2000 r.

In view of these data, a broad generalization may be made for radiation effects from a nuclear war on this country's vegetation. From 5 to 20% of the forest ecosystems may have the tree overstory seriously damaged or killed. Another 20% may be visibly affected, but without the loss of the overstory; recovery for this percentage would be relatively fast. The damage may not be fully evident for several months to a year. If fire occurred, the damage would be increased. For the rest of the country (grasslands, deserts, and tundra), temporary changes may occur in the species composition in 2 to 10% of the area, the remainder being relatively little affected.

Therefore, direct radiation effects from nuclear war on vegetation are not likely to seriously limit man's reconstruction of his renewable resources. Other ecological effects may be far more limiting, such as radioactive contamination or effects on animals and food resources.

ACKNOWLEDGMENTS

This study was supported in part by Research Contracts AT-(40-1)-2412 and AT-(40-1)-2089 from the Atomic Energy Commission, by Contract AF-31845 of the U.S. Air Force with Lockheed Aircraft Corporation, and by several grants from the Research Committee of Emory University.

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Senator Muskie. We will now hear from Dr. Cole.

(Following the hearing the letter which appears below was sent to Dr. Platt:)

January 23, 1970.

Dr. Robert B. Platt,
Chairman, Department of Biology,
Emory University, Atlanta, Ga.

Dear Dr. Platt: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass over any of the attached which are outside your area of speciality or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

Edmund S. Muskie,
Chairman, Subcommittee on Air and Water Pollution.

(The questions and answers appear in appendix IV.)

STATEMENT OF DR. LAMONT COLE, PROFESSOR OF ECOLOGY, CORNELL UNIVERSITY, ITHACA, N.Y.

Dr. Cole. Thank you, Senator.

First I would like to apologize for the form of this statement. The preparation of my statement worked out so it had to be typed on Sunday, and that means doing it myself and, as anybody can see, I am no typist.

Senator Muskie. You are a better one than I am.

Dr. Cole. I would just like to run through this giving a little emphasis in some places and to correct one possible inconsequential error on the first page here.

A good many ecologists have severe reservations about the use of nuclear explosives for excavation. I am happy to have an opportunity to talk about these.

Personally, I regard some of the proposed projects as hazardous to the point of being irresponsible. I cannot entirely blame the U.S. Atomic Energy Commission (AEC) for this because they are obligated by law to promote the "peaceful uses of atomic energy." I think there is need for a careful reexamination of the whole policy of promoting a technology that creates long-term hazards for human life and the quality of man's environment.

In principle, if a nuclear device could be exploded underground in such a way that no radioactivity would enter the atmosphere or ground water, the only immediate danger would seem to be the potential triggering of earthquakes—a possibility I am not competent to comment on. In practice, we know that some underground shots have vented when they were not intended to, but all I have been able to learn is from news reporting of the stock AEC reply: "Some radioactivity was released but it presents no hazard." I personally doubt that anyone ever knows enough about local geology to be perfectly confident that radionuclides in an underground chamber will not work their way through cracks, fractures, or porus materials and contaminate the outside environment.

There seems to be a tendency for nuclear projects to be announced and to arouse public concern before attempts are made to analyze possible hazards. For example, "Project Ketch" was to be a totally contained underground explosion in Pennsylvania designed to free and concentrate natural gas from surrounding rocks. After public protests, an AEC preliminary analysis was performed and released [1] about the predicted tritium—radioactive hydrogen, ^3H—content of the natural gas. This led to the tentative conclusion, page 27, that: "Submitting the public to this radiation exposure would result in excessive and unacceptable dose commitments."

It is very difficult to generalize about the radioactivity released by nuclear explosions. The radionuclides arise from three sources:

(1) Fission—the splitting of atoms of uranium or plutonium.
(2) Fusion—the uniting of atoms of isotopes of hydrogen.
(3) Induction—radionuclides produced by the action of the intense radiation flux on the materials of the nuclear device and surrounding materials.

Data on the contributions of these three sources are normally classified. About a year ago at Oak Ridge National Laboratory I talked with a man from Battelle Institute who, under a contract with AEC, was making calculations of the radioactivity to be expected from using nuclear explosives to dig a proposed sea level canal across Central America. He knew of my interest in the subject and expressed regret that he couldn't discuss the calculations with me because the data were classified. This military-type secrecy makes it very difficult for a scientist who is independent of AEC to evaluate the hazards of proposed projects, and in what follows I shall rely on openly published data.

Without knowing the composition of the particular nuclear device and that of the surrounding rock or soil it is fruitless to speculate about the kinds and amounts of induced radioactivity. For example, on July 6, 1962, a 100 kiloton—equal to 100,000 tons of TNT—nuclear device named "Sedan" was fired at a depth of 635 feet in the Nevada desert [2] page 57. The most significant radionuclides vented to the atmosphere by this test were various isotopes of tungsten which "are not regularly found in worldwide fallout." (See p. 6, of ORNL–TM–2229, cited in footnote 1.)

There is apparently something peculiar about the composition of that particular devise.

Nuclear fission produces a large number of radionuclides. Turner et al., 1968,[3] give a list—appendix E—of "179 radionuclides which have been of interest in the radiological safety-feasibility study for excavating a sea level canal with nuclear explosives."

In nuclear fusion the only direct radioactive product of importance is tritium (^3H), and this has led to the designation of thermonuclear devices as "clean bombs." However, a fission device is used to "trigger"

[1] Rohwer, P. S. and S. V. Kaye 1968. Age-dependent models for estimating internal dose in feasibility evaluations of Plowshare events. Oak Ridge National Laboratory, ORNL–TM–2229.

[2] Engineering with nuclear explosives. 1964. Proceedings of the third Plowshare symposium. U.S. A.E.C./Division of Technical Information. TID–7695.

[3] Turner, W. D., S. V. Kaye, and P. S. Rohwer 1968. Exrem and Inrem computer codes for estimating radiation doses to populations from construction of a sealevel canal with nuclear explosives. Union Carbide Corp., Nuclear Div., Computing Technology Center, Oak Ridge, Tenn. Rept. No. K–1752.

the explosion by creating the very high temperature necessary for fusion to take place. In addition, the radiation flux from a fusion device is about 10 times as great as from an equally powerful fission device, so the amount of induced radioactivity is much greater.

In AEC publications (for example [2]p. 132) one commonly encounters the generalization that 90 percent of the radionuclides will be trapped in the crater of an underground explosion. I have no choice but to accept this estimate although one wonders, if the crater is later flooded with water, if some of the nuclides will not dissolve and so escape into the general environment.

In view of the large number of radionuclides that may be produced in underground nuclear explosions and the uncertainty of their fate in the environment I shall confine what follows to just four nuclides which are known biological hazards and which will not be trapped in the crater because they originate as gases. At the temperature generated in a nuclear explosion—about 50 million degrees in the fireball—it would be incredible that gases could be contained in the debris. In an AEC publication which estimates that only about 10 percent of the total reaction products would be vented to the atmosphere ([2]p. 132) it is further estimated that 99 percent of the tritium would escape.

It should be kept in mind that the radionuclides under discussion can be said for practical purposes not to have existed on earth before the introduction of atomic energy. Hence we have no long-term experience with the consequences of their capacity to cause tumors, to shorten life spans, and to cause genetic damage that can affect future generations.

The nuclides I shall consider are:

(1) ^{3}Hydrogen (tritium), half-life 12.26 years. Thermonuclear processes produce enormous quantities of tritium—at least 7 million curies [4] per megaton-equivalent (equal to a million tons of TNT) of energy, and perhaps seven times that amount ([2]p. 155). The AEC publication referred to gives a range of possible values. In addition, the radiation, flux from a nuclear explosion induces the production of tritium from light elements such as helium, lithium, boron, and nitrogen in the surroundings. Tritium is also produced by fission although this was apparently not known until 1959 (see Jacobs 1968 [5] p. 2), and many recent publications overlook this. I estimate that a megaton of fission would produce 2,000 curies of tritium.

The AEC has tended to treat tritium very lightly (for example [2]p. 127) because its radioactive emanation is a beta particle of very low energy which will travel only a fraction of a millimeter in water. However, tritium becomes a constituent of radioactive water and goes everywhere that oridnary water goes including into and through the bodies of all living plants and animals. It gets built into their organic compounds including the nucleic acids which carry the genetic information for the next generation. There is recent evidence [6] that the weak beta particle from tritium may represent a greater biological hazard than much more energetic gamma radiation. It is interesting

[4] A curie of radioactivity is defined as 37 billion disintergrations per second. It is approximately the amount of radioactivity in a gram of radium.

[5] Jacobs, J. G. 1968. Sources of tritium and its behavior upon release to the environment. U.S.A.E.C./Div. of Tech. Information. TID–24635.

[6] Huver, C. W. 1969. Biological effects of tritium. Mimeo., presented to the Minnesota Pollution Control Agency, February 19, 1969.

that in the Soviet Union the maximum permissible concentration of tritium in drinking water is only one-tenth of that permitted in this country.

(2) ^{90}Strontium, half-life 28 years. One megaton of fission yields about 147,000 curies of ^{90}Sr, a beta emitter. In living systems strontium behaves much like calcium and so becomes concentrated in structures such as mollusc shells, fish scales, and the bones (and milk) of vertebrates including man. It has been regarded as the greatest long-term hazard in fallout.

(3) ^{131}Iodine, half-life 8.05 days. This nuclide emits both beta particles and gamma rays. It is estimated that a 1-megaton fission device would release at least 4 million curies of iodine. It accumulates in the thyroid gland of vertebrates and is regarded as the most immediate hazard downwind from a nuclear explosion or a reactor accident. Because of its short half-life the hazard is temporary.

(4) ^{137}Cesium, half-life 30 years. This is another beta emitter. A 1-megaton fission device produces about 157,000 curies of cesium. In biological systems it behaves like potassium which is an essential element for all living things. If present in food organisms it is passed on to the organisms eating them. In man it is most concentrated in muscle tissue.

From the above it is easy to estimate the quantities of these four nuclides (omitting tritium produced by activation) that would be produced by an explosion of known yield of either pure fission or pure fusion. However, the actual devices that would be used in excavation would be some mixture of fission and fusion. The relative quantities of the two sources of energy are apparently classified—at least I have been unable to obtain them. However, F. J. Stead of the U.S. Geological Survey ([2] p. 129) gives calculations for a device assumed to derive 99 percent of its energy from fusion and the other 1 percent from fission. I don't know whether or not AEC can actually construct this "clean" a bomb, but, again, I have no choice but to accept the figure as a basis for calculations.

On this basis a 1-megaton explosion would yield about 7 million curies of tritium, 1,500 curies of ^{90}strontium, 40,000 curies of ^{131}iodine, and 1,600 curies of ^{137}cesium. These figures agree well with those of Stead ([2] p. 129) (except that he didn't mention the iodine—he was interested in longer lived things). He further assumed that the bomb would be exploded in material having the average composition of the earth's crust and so would also yield as an activation product 10,000 curies of ^{60}cobalt, another gamma ray emitter that has been found to be highly concentrated by some biological systems. Certain clams in the Pacific got very hot with cobalt^{60}. It was so diluted in the water though that its presence was unknown.

To put these figures in the perspective of excavation, E. Graves of the U.S. Army Corps of Engineers has stated ([2]p. 323) (and again this is an AEC publication) that to excavate the sea-level canal across Panama with nuclear explosives would require a total yield of 170 megatons. With the type of "clean" device described above the yield of nuclides would be over a billion curies of tritium, about a quarter million curies each of ^{90}strontium and ^{137}cesium, and about 7 million curies of ^{131}iodine.

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The International Commission on Radiological Protection sets guidelines giving the maximum permissible burden (MPB) for the total human body for persons occupationally exposed to each radionuclide. For the nuclides discussed here the MPB's in microcuries (millionths of a curie) are 1,000 for ^{3}hydrogen, 2 for ^{90}strontium, 0.7 for ^{131}iodine, and 30 for ^{137} cesium.

On the basis of these guidelines, the billion curies of tritium released in digging this one canal would be sufficient to provide the maximum permissible whole body burden for about a trillion persons, approximately 300 times the present human population of the earth while the quarter-million curies of ^{90}strontium would equal the MPB for 125 billion persons, or about 30 times the present human population.

So here you have a dilemma. If you try to make the bomb cleaner by having nothing but fusion, you produce more tritium which is already at an unacceptable level. If you go the other way and make more of the yield fission, you produce more strontium-90 which, again, is also above what I regard as acceptable levels.

Now, these are only four nuclides. There are many more of importance here. These things pose a threat to all life including agricultural plants, livestock and seafood, and I, as an ecologist, regard the risks inherent in large-scale nuclear excavation as totally unacceptable.

I think I should emphasize again that the data I have given here on the production of tritium, of strontium-90 and cesium-137 are from Atomic Energy Commission publications. The data on the amount of tritium escaping to the atmosphere from underground explosions are from AEC publications.

Thank you.

Senator MUSKIE. You said in your opening remarks that a great deal of the information with respect to the effects of these events is classified?

Dr. COLE. Yes, sir.

Senator MUSKIE. Were you given the rationale for that classification?

Dr. COLE. No; I have talked to some of my friends who are ecologists with the AEC and they are a little bit upset about this themselves, can't imagine why it is, but nevertheless this is the case. I have sat in the room with this man from Battelle Institute where he had his computer terminal making the calculations on this thing of vital interest to me and yet we couldn't talk about his calculations.

Senator MUSKIE. So the result of that classification is that the public gets the conclusions which AEC bases upon classified information which isn't available to the public to test those conclusions. Is that an accurate statement?

Dr. COLE. Yes, sir.

Senator MUSKIE. You have said that these nuclides did not exist in any significant or perhaps in any quantities on earth until man produced them.

Dr. COLE. There were tiny amounts produced by the action of cosmic radiation, but just in the last—I can't remember the exact figure. It is a matter of 10 or 15 years. I could look it up here—the quantity of tritium in the upper Mississippi is reported to have increased by a factor of 200.

Senator MUSKIE. Do these nuclides and especially the four you mentioned have any beneficent value for mankind?

Dr. COLE. They have research value. The ecologist can benefit a little bit from them by using them to trace the flow of matter through food chains and so forth. Cobalt-60 is a commonly used source of radiation in cancer treatment.

Senator MUSKIE. How do the amounts that will be necessary for those purposes compare to the amounts that are being released?

Dr. COLE. Infinitesimal by comparison.

Senator MUSKIE. So that these nuclides which are being added to the environment of various half-life periods are almost totally risks?

Dr. COLE. Yes, sir.

Senator MUSKIE. So what we must attempt to evaluate is the balance between something man in adding to his environment, that is almost total risk, and the benefits which accrue to mankind from the activities which produce the nuclides or benefits that are generated by the nuclides themselves?

Dr. COLE. No, sir. I visited just a couple of weeks ago at NYU medical center and some of their toxicologists there were talking about one of these very points, the very small attention that has been given to tritium in the environment, and one of these toxicologists gave it as his opinion that this is perhaps the most dangerous of all the nuclides because it does get right into the genes, and tritium in some types of organic material will actually be accumulated preferentially in the DNA, the nucleic acid which comprises the genetic code.

Senator MUSKIE. It seems to me that the release of these nuclides into the environment has the net effect of growing environmental degradation in terms of the environment's capacity to sustain life.

Dr. COLE. And we don't know how bad a risk these things really are because, well, Dr. Platt mentioned something about it taking several years to find out. When you are talking about genetic damage it can take several generations to find out.

Senator MUSKIE. One area that we haven't really discussed at these hearings thus far is the question of the ways in which these nuclides find their way into the environment, ways that perhaps haven't been fully appreciated until now.

I wonder if you or Dr. Platt would add to the record on this point?

Dr. COLE. That is the reason I confine my attention to these four, because they are produced as gases that will escape. Now, others move in different pathways. Some are soluble in water, some of them adhere to dust particles and are carried in the atmosphere and so forth. Tritium is probably the most easily moved in the environment of all the isotopes.

At Oak Ridge, for example, they are testing on watersheds the ability of the soil to trap and retain these isotopes and certain types of clay in many soils almost totally bind cesium-137, where in the water it circulates through living things. If you pour it on the soil surface of the right kind of soil, it doesn't move, but the tritium moves just as easily as ordinary water.

Senator MUSKIE. I share with Dr. Platt his concern about the detrimental effects of the beta emission.

Dr. COLE. Yes. It seems to me, though, that this isn't quite as recent a finding as I think he implied because we recognized a good many

years ago that strontium-90, which is a beta emitter, and doesn't emit any gamma, was really a hazard involved and was very alarming.

Senator MUSKIE. Is that a concept widely shared now in the scientific community?

Dr. COLE. Oh, yes. I think so.

Senator MUSKIE. Did you want to comment, Dr. Platt?

Dr. PLATT. A point of clarification. My reference to beta had to do with direct effects from fallout. Dr. Cole is perfectly correct that beta emissions from radionuclides absorbed in living systems has been of great concern from the very early days of the atomic age.

Senator MUSKIE. I see. And you also share with Dr. Platt your concern about excavation as the most dangerous of the kind of Plowshare activities which are being explored and contemplated in terms of the safety to human beings?

Dr. COLE. Yes, and unless I have made some awful mistake in my calculations—and I have looked very carefully to try to find it—it seems to me that this one canal that is proposed would have a quite unacceptable effect on the world environment. Those gases are going to fall out in the oceans and pass into the food chains of the oceans and it may be pretty nearly anywhere.

Senator MUSKIE. I take it from what you gentlemen have said that you would not agree that the area of environmental health is a good place in which to make budget cuts in light of these concerns?

Dr. COLE. Quite the opposite.

Senator MUSKIE. You would not applaud the $30 million cut made by the administration's budget in this field. I don't want to inject a political note here, but I think we ought to have our eyes on what is happening in the funding of these programs. I suppose it is relatively easy as one goes through a budget to cut the less visible items and concentrate on the more visible items.

I would assume you would rather see this cut restored, perhaps even at the expense of the SST?

Mr. COLE. That is another story; I would be glad to talk about but I don't think it is relative to this.

Senator MUSKIE. I don't intend to put you on the spot. I just couldn't resist the observation.

Gentlemen, thank you very much for your testimony. It has been a very helpful morning for us. I think you have added some important pages to the record.

Mr. COLE. It has been a pleasure.

Dr. PLATT. Thank you, Senator Muskie.

(The following letter was later sent to Dr. Cole:)

JANUARY 23, 1970.

Dr. LAMONT COLE,
Professor of Ecology,
Cornell University,
Ithaca, N.Y.

DEAR DR. COLE: I wish to express my appreciation to you for your informative statement before the Subcommittee on Air and Water Pollution on the potential environmental effects of underground uses of nuclear energy. Your statement will be of valuable assistance in improving public understanding of the environmental consequences associated with this technology.

As I indicated during the hearings, additional questions would be submitted to complete the records and prepare for additional hearings. Please feel free to pass

over any of the attached which are outside your area of speciality or to which you do not wish to respond.

Your early response to this request would be appreciated.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

(The questions and answers appear in appendix IV.)

NATIONAL RADIATION HEALTH STANDARDS—A STUDY IN SCIENTIFIC DECISION MAKING*

(By Michael Goodman**)

I

"The narrower the field in which a man must tell the truth, the wider is the area in which he is free to lie. This is one of the advantages of specialization."[1]

President Eisenhower's farewell speech warned of the growing dominance of our nation's scientific-military-industrial complex. But Eisenhower uttered a second warning. A warning about the newfound relationship between the scientist and the administrator.

We may have heard more about the scientist-soldier than about the scientist-manager, but the latter is equally threatening to our democratic community. When a scientist is a soldier, he is subject to direction and is a means to an end established by someone else. When he is a manager, he sets the goals and directs society. This case study concerns the vital goal of national radiation health standards and the scientific conflict over who shall be the managers.

The controversy does not center over personalities such as the Tizard-Lindemann conflict in World War II[2] but over scientific concepts. Two schools of scientific thought exist over the hazards of radiation exposure to man. One group argues that there is a safe permissible level below which no danger occurs to man (threshold concept). The second group holds that damage from radiation increases proportionately to the amount of radiation exposure which resulted (linear concept).

The discussion will consider two categories of cells in man. Those concerned with the maintenance and integrity of the individual (such as cells in bone marrow, blood, liver or nervous system) and those concerned with the maintenance and integrity of the genetic information that is handed on from generation to generation (reproductive cells of gonads). Correspondingly, we shall speak of *somatic effects* (limited to the irradiated organism itself), and of *genetic effects* (limited to its descendants).[3]

Both scientific groups accept these potentially dangerous threats to man's existence. But both groups disagree over the setting of radiation standards and the implementation of procedures. The issue is still in the balance. Short run effects of a final decision will have military consequences plus affect the feasibility of a profitable commercial application of atomic energy by corporate enterprise. The long-range effects include the very life of man.

[This case study gives us the opportunity to observe the role of the Executive Bureau's Office of Science and Technology (OST) and its genesis, the Special Assistant to the President for Science and Technology. The problem cuts across government from the Public Health Service and Department of Health, Education, and Welfare to the Atomic Energy Commission. The relative role of the OST in coordinating the differing governmental views will be considered.[4]] An historical perspective is now needed to fully develop the complexities of the controversy and the institutionalized framework of the contending parties.

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**Legislative assistant to Congressman William J. Randall, Democrat from Missouri.

[1] Robert M. Hutchins and others, *Science, Scientists, and Politics*, Center for the Study of Democratic Institutions (Santa Barbara, Cal., 1963) p. 1.

[2] C. P. Snow, *Science and Government* (Cambridge, 1960).

[3] *Report of the United Nations Scientific Committee on the Effects of Atomic Radiation*, General Assembly, Official Records: Thirteenth Session Supplement No. 17 (New York, 1958) p. 5.

[4] Any thorough analysis of OST is impossible. It is hindered by what Arthur M. Schlesinger, Jr. calls the "Age of the Telephone." Conversation with Ralph Lapp, Science Consultant, on April 3, 1963. (Editorial Note).

II

Mme. Marie Curie and her husband were the first to understand natural radiation and that radiation causes death was proved by the experience of Mme. Curie herself.[5] The first portent of the hazards of radiation came with the development of the medical x-ray. The first reported use in the United States was made by Dr. Emil H. Grubbe on January 29, 1896. Shortly thereafter, he reported: "I had developed dermatitis on the back of my left hand that was so acute that I sought medical aid." Subsequently, he helped instruct more than 7,000 doctors in radiological techniques—the x-ray specialists of the United States of the early 1900's. Regarding these 7,000 doctors, Dr. Grubbe stated: "Many of them have died of radiation; I tried to warn them, but they didn't listen."[6] By the year 1900, there were 170 cases recorded in which x-rays had produced biological damage to individuals. A number of studies were published prior to World War I of the injuries caused to those who were using and abusive the x-ray.[7]

As a result of the widespread use of x-rays to locate schrapnel in the military hospitals during World War I, the death rate of radiologists increased sufficiently to alarm the profession and bring about the formation of Radium Protection and X-Ray Committees. These committees set exceedingly high limits as permissible for their colleagues of the x-ray profession. In France in 1924, the suggested maximum permissible limit of exposure to x-ray for doctors was 4,000 roentgens.[8] Earlier a figure of 3,000 a year[9] or approximately 10 roentgens per working day was proposed in Germany.[10] X-ray experts continued to die from radiation exposure. In 1927, a much lower standard of 300 roentgens a year—or one roentgen per working day—was adopted as a voluntary limit by the United States X-Ray and Radium Protection Committees.

Action to stem the growing rate of death caused by overexposure to radiation came in 1928 with the convening of the second International Congress on Radiology in Stockholm, Sweden. Various national committees on x-ray and radium received invitations to attend. The American Roentgen-Ray Society and their competitive organization, the Radiological Society of North America, sent representatives. Conflict soon appeared. "Each of our two radiological societies offered recommendations, and each claimed to be the authoritative body."[11] The remarks were attributed to a young physicist named Lauriston S. Taylor. He was representing the National Bureau of Standards of the U.S. Department of Commerce as an observer.

The Congress adopted a series of British recommendations for a maximum permissible limit. In the process, the U.S. delegates "showed up rather poorly, in that agreement could not be reached on who authoritatively represented the views of the U.S."[12] A final conference decision was made to set up an International Commission on Radiological Protection (ICRP). With the U.S. situation in mind, the elected chairman of the ICRP, Dr. G.W.C. Kaye, recommended that a single central committee be established within those countries having more than on radiological organization. Dr. Kaye's suggestion was made to facilitate the consolidation of national recommendations for the next meeting of the ICRP.[13]

III

Dr. Lauriston S. Taylor took the leadership in creating a national committee. Taylor's leadership was strengthened by the fact that the National Bureau of Standards (NBS) had established a definite long-range program in the general

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[5] Jack Schubert and Ralph Lapp, *Radiation: What It Is and How It Affects You* (New York, 1957), p. 20.

[6] Washington *Daily News*, Thursday, July 16, 1959, p. 22. (An unsigned news story).

[7] Dr. Percy Brown, *American Martyrs to Science Through the Roentgen-Ray* (Baltimore, 1936); Dr. O. Hesse, *Radiation Induced Injuries* (New York, 1911).

[8] Ethel Browning, *Harmful Effects of Ionizing Radiation* (London, 1959), p. 20.

[9] Professor H. Holthusen, "General Principles for Medical Supervision of Workers," a paper presented at Euratom Conference at Stresa, Italy, May 2, 1961.

[10] Roentgen, the (R.)—That quantity of X- or gamma radiation required to produce in dry air (1 c.c.) ions carrying 1 electrostatic unit of positive or negative charge. Note—The absorbed dose (in tissue) expressed in rod, multiplied by the RBE (Relative Biological Effectiveness) for the type of radiation concerned, gives the biologically effective dose in rem.

[11] Lauriston Taylor, "A Brief History of the National Committee on Radiation Protection and Measurements," *Health Physics* (June, 1958), p. 3.

[12] Ibid.

[13] Ibid., p. 4.

field of radiological protection. The Bureau had the only laboratory in the country having as its primary interest the development of radiological protection data. Lastly, Dr. Taylor and the NBS had no inter-society or political ties and therefore could be expected to retain an "independent position and viewpoint."[14] Early in 1929, the Advisory Committee on X-ray and Radium Protection was established with Lauriston S. Taylor as its head.

The Advisory Committee was renamed and restructured at a December 4, 1945 meeting. It was renamed the National Committee on Radiation Protection (NCRP). The reorganization called for the creation of an executive committee, main subcommittee and as many subcommittees as necessary to consider the problems involved. The final structuring found the elected chairman in a position of rare power and influence. The Executive Committee was composed of five members appointed by the Committee Chairman who also acted as its head. The appointments were subject to the approval of the Main Committee.

The Main Committee would be composed of a) technically qualified representative appointed by organizations interested in the scientific and technical aspects of radiological protection, b) representatives at large whose services were felt to be of special value, and appointed by the Executive Committee, and c) the chairmen of the various subcommittees; all subcommittee chairmen were appointed by the Committee Chairman with the approval of the Executive Committee. The subcommittee chairmen were allowed to choose their own working groups but the final reports would be submitted to the Executive and Main Committees for approval.[15]

Nominations for the Committee Chairmanship were thrown open at that December 1945 meeting. Dr. Lauriston S. Taylor was nominated and approved by vote to continue indefinitely.[16]

Dr. Taylor is still Chairman of the NCRP. From his position as Chief of Atomic Radiation Physics at the Bureau of Standards, he has turned the self-appointed National Committee on Radiation Protection into a quasi-governmental organ. The Committee, lacking any legal statutory base, is now viewed as the major regulatory body in checking the dangers of radiation. Taylor's membership on the ICRP plus chairmanship of the International Commission on Radiological Units gives him unparalleled influence in this vital area of health and safety.

The NCRP's regulations, standards and rules for working conditions and air and water concentrations are published by the U.S. Department of Commerce. They are known as National Bureau of Standards Handbooks Nos. 42, 48, 49, 50, 51, 53, 54, 55, 57, 58, 59, 60, 63, 65, 66, 69, 72, 73, 75, 76, 79 and 80. These handbooks contain thousands of calculated maximum permissible body burdens and maximum permissible concentrations of radionuclides.[17]

The ICRP's very first acceptance of the threshold concept of radiation was adopted by the NCRP. The ICRP's recommendations for occupational exposure limits for doctors, technicians or industrial workers were scrupulously followed by the NCRP as they were reduced in 1935 to 100 R, 65 R in 1945, 34 R in 1950 to 15 R in 1955.[18] In 1960, these occupational exposure levels were dropped for a series of complicated formulas dealing with the body's skin and critical organs.[19]

The ICRP sets its recommendations with some trepidation. In its official report published in 1955, the ICRP stated:

"that whilst the values proposed for maximum permissible doses are such to involve a risk which is small compared to the other hazards of life, nevertheless, in view of the incomplete evidence on which the values are based, coupled with the knowledge that certain radiation effects are irreversible and cumulative, it is strongly recommended that every effort be made to reduce exposure to all types of ionizing radiation to the lowest possible level. The Commission will continually review the recommended permissible levels as new information becomes available."[20]

[14] Ibid., p. 5.
[15] Ibid., p. 6.
[16] Ibid.
[17] Radionuclide—A radioactive nuclide. Nuclide—An Atom of a particular; that is characterized by an atomic number and an atomic weight.
[18] R stands for roentgens.
[19] "Radiation Protection Criteria and Standards: Their Basis and Use," Summary-Analysis of Hearings before the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, Congress of the United States, 86th Cong., 2nd Sess., Appendix 5, p. 60.
[20] "Recommendations of the International Commission on Radiological Protection," Supplement No. 6, *British Journal of Radiology* (London, 1955), p. 10.

This cautious approach was belied by Dr. Taylor in a speech to a UNESCO conference on "Radioisotopes in Scientific Research" held in Paris, France, in 1957. Dr. Taylor bluntly said:

"It may perhaps sound startling to contend that the establishment of permissable levels of radiation exposure is not basically a scientific problem. Indeed, it is more a matter of philosophy, of morality, and of sheer wisdom."[21]

Taylor went on to elaborate his philosophy on radiation:

"Thus all indications seem to point to the fact that present and future solutions of the radiation protection problem will have to be based on a risk philosophy; they will have to be compromise solutions, and cannot be solved on the basis of scientific evidence alone."[22]

His risk philosophy was reduced to dollar and cents terms when he commented that a high safety standard for workers would have a marked impact on plant management. "It could be very costly and could seriously retard the atomic industry."[23] A theoretical atomic industrial hazard situation is stated below.[24]

A new dimension to the problem of radiation safety was added in the early 1950's. Stimulated by passage of the Atomic Energy Act of 1954 and supposed break-throughs in nuclear reactor technology, a group of public utilities formed the Power Reactor Development Company (PRDC) for the construction of a 300 megawatt reactor at Lagoona Beach, Michigan.

The atomic energy industry took notice. PRDC planned to take the unheard of action of constructing a major nuclear installation near a great metropolis, Detroit. A fast-breeder reactor was proposed by PRDC—a design which held the monetary advantages of generating power while producing plutonium. But this very design had gone critical in the isolated wastes of Utah.

Questions on the reactor's safety were raised. To block possible objections, PRDC contracted with the University of Michigan for a series of studies showing the reliability of its nuclear reactor. The AEC followed suit. The Joint Committee on Atomic Energy (JCAE) evinced concern. JCAE Chairman Carl T. Durham asked for a formal statement of the AEC's position.[25] Senator Clinton P. Anderson stated, "I felt that the AEC should have paid more attention to the warning of the reactor safeguards committee on the Lagoona Beach reactor design."[26]

PRDC's actions with the approval of the AEC posed a particularly serious problem for the NCRP. The NCRP had no governable standards for populace living in the environs of a major nuclear installation as proposed for Lagoona Beach. Heretofore, the NCRP had taken its standards from its parent International body, the ICRP. In 1955, the ICRP had recommended only an occupational exposure limit and a maximum permissible level (MPL) for the general population. The latter was caused by world anxieties over nuclear fallout. The ICRP recommended that for large populations the MPL "should be reduced by a *factor of ten* below those accepted for occupational exposures."[27]

The NCRP was pressed for answers. Senator Anderson forced the issue by calling the unions and telling them "that here was a reactor being built next to a great metropolitan area where workers in other factories would be subjected to the harmful radiation effects if there should be a breakdown in the Lagoona reactor."[28] The Unions did intervene and PRDC faced serious opposition.

[21] Lauriston S. Taylor, "Radiation Exposure and the Use of Radioisotopes," *The Impact of Science on Society*, Vol. VIII (1957), No. 4, published by UNESCO, Paris, France, p. 210.
[22] Ibid., p. 214.
[23] Ibid., p. 216.
[24] "The control of radiation hazard will add significantly to the total cost of building and operating a nuclear power plant. The cost of designed-in control safety must be evaluated in terms of the savings they will permit in operating costs. Construction costs are paid only once, but operating costs will be paid throughout the life of the plant. Suppose that a piece of equipment has failed in an area of high radiation levels, and that replacement of the equipment will require two man-hours of work in an area where the radiation level is 10 r per hr. (r-rem, roentgen equivalent; see footnote 10, above.) Suppose also that the policy for special working conditions in the plant permits a man to receive 2 r in a single exposure, provided that he is removed from radiation exposure for a period of 13 weeks. Under these conditions, the replacement job can be done with a minimum of 10 men, each receiving 2 r in a 12 minute work period. For the next 13 weeks these 10 men will be excluded from radiation work. If there is no radiation-free work in the plant for these men to do, the cost of the replacement job can be as high as 2½ man-years." G. Hayt Whipple, "Radiation Hazard Control for a Power Reactor," presentation at the Nuclear Engineering and Science Congress sponsored by *Engineers Joint Council*, Dec. 12-16, Cleveland, Ohio, p. 4.
[25] Letter to the Atomic Energy Commission, July 6, 1956.
[26] Speech by Senator Clinton P. Anderson before the Second Annual Convention of the New Mexico State AFL-CIO, Oct. 26, 1957, p. 9.
[27] "Recommendations of the International Commission on Radiological Protection," p. 10.
[28] Speech by Senator Clinton P. Anderson, October 26, 1957, p. 9.

The NCRP answered the problem of installation environs by issuing an addendum to Handbook 59 on April 15, 1958. The addendum stated that people living in the environs of a major nuclear installation could be subjected to thirty per cent of the occupational exposure limit without any harmful effect. The NCRP did this with no prior consultation or prior recommendation of the ICRP. It was an arbitrarily decided decision.[29] This action was not lost on the nation's scientific community.

Answers became complicated with the publication of the PRDC and AEC reliability studies for the Lagoona reactor. In answering the request of JCAE Chairman Durham for factual data on its safety, Harold S. Vance, Acting Chairman of the AEC, concluded in an accompanying letter to their study [30] that: "more than 100 reactor years of regular operating experience have been accumulated, including experience with reactors of high power and large inventories of fission products, without a single personal injury and no significant deposition of radioactivity outside the plant area." [31]

The NCRP thought differently. In hearings before the JCAE on the nature of radioactive fallout for man, a chart was inserted with the testimony of Dr. Lauriston S. Taylor. The chart titled the "Current Situation with Regard to Permissible Radiation Exposure Levels" listed "installation environs" as a major radiation source.[32]

The NCRP, PRDC and AEC studies all endorsed the location of the reactor near a major metropolis. But both reports disagreed over the theoretical level of destruction and damage if the proposed plant ever went critical. The AEC summary statement cited figures of 3,400 killed and 43,000 injured.[33] The PRDC report prepared by the Engineering Institute of the University of Michigan was less optimistic, specifying 133,000 killed.[34] But again, both reports gave assurances that no major reactor accident could ever occur. PRDC's report considered it "to be an incredible event!" [35] An event which would have to be multiplied by "the probability that the energy release ruptures *all the containment provided*. Both of the latter are very close to zero, so the real probability is virtually zero." [36] (Emphasis added.) The AEC's study cited the probability of major reactor accidents as ranging from "one in 100,000 to one in a billion." [37]

An "incredible event" prevented the entire PRDC controversy from descending into a technical quagmire over installation location, relative radiation standards and reactor design feasibility. It was the Windscale incident in England.

The British Atomic Energy Authority reported that the Number One Pile at Windscale went critical on October 10, 1957. The Pile had overheated causing a uranium fire and the accidental overspill of radioactive atomic fission products over a region.[38] The disaster bore special significance for PRDC since the reactor involved was an air-cooled fast-breeder model. The one chance in a billion had happened.

The full story of the Windscale disaster has never been fully disclosed. The British White Paper admits that all the reactor containment features provided failed. The uranimum thermocouples failed to work. The Pile power meter read

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[29] *Maximum Permissible Radiation Exposures to Man*, Addendum to National Bureau of Standards Handbook 59, *Permissible Dose From External Sources of Ionizing Radiations*, April 15, 1958.

[30] *Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Power Plants*, United States Atomic Energy Commission (March 1957).

[31] Ibid., p. vii.

[32] "The Nature of Radioactive Fallout and Its Effects on Man," hearings before the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, Cong. of the U.S., 85th Cong., 1st sess., May 27–29 and June 3, 1957, Part I, p. 813.

[33] "Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Power Plants," p. viii.

[34] The Detroit *Free Press*, September 19, 1958.

[35] *A Report on the Possible Effects on the Surrounding Population of an Assumed Release of Fission Products into the Atmosphere from a 300 Megawatt Nuclear Reactor Located at Lagoona Beach, Michigan*, The Engineering Research Institute, University of Michigan (July 1957) Foreword.

[36] Ibid.

[37] *Theoretical Possibilities and Consequences of Major Accidents in Large Nuclear Plants*, p. viii.

[38] Arthur H. Wolff, D.V.M., "Mild Contamination in the Windscale Incident," *Public Health Reports* (January 1959) p. 42.

low. The scanning gear jammed and the fuel cartridges burst.[39] In fact. the fire in the Pile was in progress for 24 hours before adequate diagnosis of the situation was made.[40] The reactor was finally brought under control by forced water injection which destroyed the core.[41]

Windscale is located in the northwest corner of England on the Irish Sea. Visualize an overheated air-cooled reactor spewing out quantities of fission products through its 400 feet high exhaust chimneys under favorable climatic conditions. The effect was described by a local medical officer:

"The wind and climatic conditions at the time were very material to the consequences. It was said at first that a northeast wind had blown the fission products safely out to sea, but the statement was untrue. Within a short time the wind was certainly seen to be from the northwest and the subsequent geographical pattern of the fallout confirmed it so. There was also fairly heavy rain on central and southwest Lakeland mountains early that evening so it seems possible that there was an additional washout of the cooling airstreams. Some of the airborne fission products were traced in London and tellurium was detected in Denmark.[42]

The medical officer continued his observations:

"The height of the chimneys, the prevalence of northwest and west winds, the general movement of air masses in low pressure systems, the precipating effect of the mountains, and the air canalisation in valleys, raise doubts as to whether their chronic and cumulative effects may not be as important to public health in areas within some 35 miles downwind as they are in the immediate shadow of the reactors." [43]

His doubts proved to be correct. The authorities were compelled to seize all milk and all growing foodstuffs in a 400 square mile area around Windscale. The significance of this act must be viewed in the context of maximum permissible limits established by the ICRP. There are many different radionuclides contributing to the usual spectrum of gross fission products.[44] The one that has received the most public attention is strontium-90, but it is not to well appreciated that there are other radionuclides to be considered in evaluating public health aspects of nuclear fission. Radioactive iodine, in particular Iodine-131, is a significant component of freshly produced fission products. At Windscale a disproportionate amount of the Iodine, because of its volatility, was driven off as a result of the uranium fire in the reactor.[45]

This released Iodine-131 fell on pasture land in the region. Cattle feeding in the fallout area soon produced milk with high levels of radioactivity. The British realized that they were confronted with a serious problem of milk contamination for which there were no standards of emergency permissible levels that could be applied. The British did not look to the ICRP for guidance but set up an emergency permissible level of 0.1 microcurie [47] of Iodine-131 per liter of milk and set up a widescale sampling program to delineate the affected milksheds.[48] It was necessary to condemn about 250,000 gallons of milk from about 600 herds of cattle. The population of the area was approximately 100,000 people.[49]

The Windscale nuclear accident left its impact on British public health authorities. One comment was:

"The public health authorities, so deeply absorbed in pursuing the minutise of their personal services, must now recognize that they have serious responsibilities in the environmental field for safeguarding against the increasing risks of radioactivity. Because the production of radioactive materials is now many thousands times greater than it was in the last decade, the public health authorities cannot afford the complacency of leaving all control to the producers of fission energy and to the atomic physicists." [50]

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[39] "The Accident at Windscale No. 1. Pile on 10th Oct., 1957," *Command Paper 302* (November 1957) p. 2.

[40] Wolff, p. 42.

[41] "The Accident at Windscale No. 1 Pile on 10th October 1957," p. 3.

[42] Frank T. Madge, Mid., "Over the Hills from Windscale," *Lancet* (November 1957) p. 2.

[43] Ibid., p. 3.

[44] Paper 316, The Second United Nations International Conference on Peaceful Uses of Atomic Energy (July 18, 1958).

[45] See F. 17.

[46] Wolff, p. 42.

[47] Microcurie—One-thousandth of a curie.

[48] Wolff, p. 42.

[49] Ibid., p. 43.

[50] Madge, p. 1.

IV

The progress of science through experiment and application has always been darkened by innocent and inevitable victims. The crew of the Japanese fishing trawler *Lucky Dragon* were such victims.

On March 1, 1954, the crew watched the sun rise in the west.[51] An atomic bomb was detonated on Bikini Atoll approximately 85 miles away. On March 2, a light rain or drizzle started to fall. "Tiny bits of sandy ash came swirling down onto the decks. Later ash drifted down and touched lips, deposited on earlobes and dusted men's hats. The crew felt pains in their eyes."[52] The crew was being subjected to nuclear fallout.

In examining the plight of the *Lucky Dragon* crew, Ralph Lapp said, "The basic difficulty is that medical science is ignorant of what really happens when radiation flashes through body cells and somehow or other leaves its stealthy stamp on the issues."[53] There is "no precedent in medical science for evaluating the impact of radiation which penetrates the whole body."[54]

The death of one crew member and the prolonged hospitalization of the others closed one of the more unfortunate chapters in man's scientific progress.

Scientific concern over radiation standards was magnified by the *Lucky Dragon* incident. The National Academy of Sciences undertook a series of studies to analyze the conceptual problem in radiation. Their studies were made public in 1956.

One study cautioned that "we should not disregard a danger simply because we cannot measure it accurately nor underestimate it simply because it has aspects which appeal in differening degrees ot different persons."[55] The final Summary Reports of the National Academy of Sciences were particularly illuminating. On the somatic effects of radiation, the Summary Reports said:

"The quantitative relations are not yet clear, but it is established that certain malignancies such as leukemia, and certain other cellular abnormalities can be induced by ionizing radiation. There is also some evidence that effects of this sort can measurably shorten the life expectancy of the individual receiving the radiation."[56]

The Summary Reports then discussed the possible congenital damage to future generations:

". . . the best index of genetic damage is the totality of tangible genetic defects of living individuals—say such things as mental defects, epilepsy, congenital malformations, neuromuscular defects, hematological and endocrine defects, defects in vision or hearing, cutaneous and skeletal defects, or defects in the gastro-intestinal or genitourinary tracts. Roughly 4–5% of all live births in the United States have defects of this sort; and of all these, perhaps about half—or 2% of the total live births—have simple genetic origins and appear prior to sexual maturity. If mankind were subjected to a 'doubling dose' of radiation, then the present level of 2% of such genetic defects would rise, and would eventually be doubled. More explicitly, consider the next one hundred million births in the United States. This is about the number of children that will, in the future, be born to the presently alive population of the U.S. Of these 100,000,000 children, something like 2,000,000 will experience genetic defects of the sort listed, these resulting from the deleterious 'spontaneous' mutant genes which have been induced by natural causes excluding man-made radiation. If we were to be subjected, generation after generation, to an additional doubling dose of man-made radiation, then the present tragic figure of 2,000,000 would gradually increase by 2,000,000 more cases up to an eventual new total of 4,000,000."[57]

In a pointed reference to the threshold concept of maximum permissible limits, the Summary Reports concluded:

"It has been thought that there may be a *rate* (say, so much per week) at which a person can receive radiation with reasonable safety as regards certain types of direct damage to his own person. But the concept of a safe rate of radia-

[51] Ralph E. Lapp, *The Voyage of the Lucky Dragon* (New York 1957) p. 32.
[52] Ibid., p. 34.
[53] Ibid., p. 111.
[54] Ibid., p. 121.
[55] *The Biological Effects of Atomic Radiation: A Report to the Public*, National Academy of Sciences—National Research Council (Washington, 1956) p. 15.
[56] *The Biological Effects of Atomic Radiation:* Summary Reports, National Academy of Sciences—National Research Council (Washington, 1956) p. 15.
[57] Ibid., p. 25.

tion simply does not make sense if one is concerned with genetic damage to future generations. What counts . . . is the *total accumulated dose to the reproductive cells of the individual from the beginning of his life up to the time the child is conceived.*"[58]

The Summary Reports were signed by sixteen noted scientist.[59]

The National Academy of Sciences Reports were followed in 1958 by the publication of a study by the United Nations Scientific Committee on the Effects of Atomic Radiation. This Committee's conclusions were analogous to the National Academy of Sciences Reports. The Committee's publication emphasized "that radiation-induced mutations are in general, harmful and increase in direct proportion to the genetically significant exposure, even at very low dose levels."[60]

Against this rapidly forming scientific consensus, advocates of the threshold concept continued to set maximum permissible levels of exposure and present their scientific justifications to international meetings. One presentation by Dr. Karl Z. Morgan of the ICRP proved especially interesting. Addressing the Second United Nations Conference on Peaceful Uses of Atomic Energy, Dr. Morgan commented on the data used in revising internal dose recommendations for the ICRP. He freely admitted that human metabolic information was lacking in setting standards. Dr. Morgan then stated that animal data was often used and almost without exception the data was "obtained from only single dose experiments."[61]

VII

It was against the background of the controversial PRDC installation, the Windscale incident (which was noted in American health journals[62]), and the public disclosure of authoritative scientific findings that Dr. Leroy E. Burney, Surgeon General of the United States, decided to take action.

On February 19, 1958, the Surgeon General announced the formation of the National Advisory Committee on Radiation.[63] Dr. Burney commented that "the development of adequate safeguards against the hazards of radiation must be regarded as an increasingly important public health responsibility."[64] The Public Health Service (PHS) press release cautioned that the new Committee should not be confused with the National Committee on Radiation Protection and Measurement (NCRP added measurement to its title at some undisclosed moment).[65]

The members of the newly formed Committee were Dr. Arnold O. Beckman, President of Beckman Instruments of California; Dr. Victor P. Bond, Pathology Division—Medical Department, Brookhaven National Laboratory; Dr. Richard H. Chamberlain, Professor of Radiology, Hospital of the University of Pennsylvania; Dr. James F. Crow, Professor of Genetics, University of Wisconsin; Dr. Herman E. Hilleboe, Commissioner of Health, New York State; Dr. Edward B. Lewis, Professor of Biology, California Institute of Technology; Dr. Berwyn F. Mattison, Executive Secretary of the American Public Health Association; Dr. Russell H. Morgan, Professor of Radiology, John Hopkins University, Medical School and Radiologist-in-Chief, John Hopkins Hospital; Mr. Lauriston S. Taylor, Chief, Atomic Radiation Physics Division of the National Bureau of Standards; Dr. George W. Thorn, Physician-in-Chief, Peter Bent Brigham Hospital; and Dr. Abel Wolman, Professor of Sanitary Engineering, Johns Hopkins University. Dr. Russell H. Morgan was designated Chairman of the National Advisory Committee on Radiation.[66]

The nomination of Lauriston S. Taylor brought the acknowledged scientific spokesman for the threshold concept to NACOR. The views of Chairman Morgan were not widely known. He did speak to the Association of State and Territorial Health Officers in 1957, voicing fears of an inadequate federal-state radio hygiene program.[67] "This is a problem of major magnitude," he noted. "There is no

[58] Ibid., p. 17.
[59] The signers: Warren Weaver, Chairman, George W. Beadle, James F. Crow, M. Demeree, G. Failla, H. Bentley Glass, Alexander Hollaender, B. P. Kaufman, C. C. Little, H. J. Miller, James U. Neal, W. C. Russell, T. M. Sonneborn, A. H. Sturtevant, Shields Warren, Sewall Wright.
[60] Report of the United Nations Scientific Committee on the Effects of Atomic Radiation, p. 34.
[61] Dr. K. Z. Morgan, "Summary of Data Used in Revising Internal Dose Recommendations of the ICRP," *Second United Nations Conference on Peaceful Uses of Atomic Energy*, Vol. 21 (1958), p. 10.
[62] Wolff, p. 1.
[63] Atomic Industry Reporter, February 19, 1958, p. 4:59.
[64] Ibid.
[65] Ibid.
[66] Ibid.

question that the radio hygiene problem will grow with the normal expansion of nuclear energy." [68] Dr. Morgan then concluded that "enormous amounts of knowledge were yet to be uncovered in the field of radiation." [69]

The National Advisory Committee on Radiation held its first meeting on March 13, 1958, and submitted its final report to the Surgeon General and to the public one year later. The intervening period provides an illuminating look at the development of a scientific concensus against the exigencies of government.

VIII

The year 1959 proved to be a critical and exhaustive one for exponents of a major application of atomic energy in the United States. No less than six major sets of hearings were held on different aspects of atomic energy. Hearings examined industrial radiation waste disposal, employee radiation hazards, federal-state cooperation, fallout from nuclear weapons testing, development of the atomic energy industry plus the customary AEC authorization hearings. These hearings provide a critical backdrop for discussing the March 1959 NACOR report.

Criticism of the NCRP was leveled from different quarters. Dr. Jack Schubert, senior chemist, Division of Biological and Medical Research, Argonne National Laboratory and member of numerous NCRP subcommittees questioned the Committee's composition:

". . . in order to achieve the best possible estimates . . . a new committee of scientists representing all the pertinent fields of science, such as genetics, biostatistics, medical science, physics and so forth, should be created and the membership be chosen by some procedure recommended by an organization such as the National Academy of Sciences. . . . It is important such a committee, which must now deal with an entirely new problem, the radiation effects on the whole population, not be dominated by just a few fields of specialty. The National Committee on Radiation Protection has dealt many years with the problems of permissible levels before any of us, including myself, had ever considered that such a problem existed. However, in the past five years because of the newness of the problem that I just mentioned, it seems to me that the NCRP does not meet the scientific requirements which I have just stated and its basis for selecting committee members is inappropriate to this new problem." [70]

Rising levels of radioactivity in our nation's milk and foodstuffs provoked another controversy for the NCRP.[71] Laborite Konni Zilliacus rose on the floor of the British Parliament and complained that American wheat had been so contaminated by strontium fallout from atomic tests that it should be barred from Britain. Zilliacus said 1958 samplings of U.S. wheat averaged 50 percent above the AEC's safe limit, a substance which concentrates in bone.[72]

Criticism began when the NCRP announced that major revisions had been made in the maximum permissible limits of a large number of radioactive substances that find their way into water, air, food and the human body. In the announcement, "Karl Z. Morgan, Chairman of the Subcommittee on Permissible Internal Dose, said that the strontium-90 limit was doubled because it was figured on a new basis." [73] Major criticism broke when it was discovered that the NCRP had split with the ICRP, its parent organization.

The *Washington Post* learned that the ICRP had lowered the permissible concentrations of radioactive strontium-90 to a level which was already being exceeded in milk and foods in some parts of the United States.[74]

The *Post* commented that:

". . . the split between the two groups over the 'permissible' limits for general populations had been the first major one since the groups were established. They have made a great effort to stay together heretofore in order to avoid the confusion that would result from different standards." [75]

[68] Ibid.
[69] Ibid.
[70] "Fallout from Nuclear Weapons Tests," hearings before the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, Congress of the United States, 86th Congress, 1st Session, May 5, 6, 7, and 8, 1959, Vol. 2, p. 1607.
[71] *Washington Post*, October 16, 1958; January 5, 1959; February 25, 1959; March 1, 1959; March 3, 1959; March 5, 1959; and March 7, 1959.
[72] *Washington Post*, February 25, 1959.
[73] *Washington Post*, April 23, 1959.
[74] *Washington Post*, April 25, 1959.
[75] Ibid.

The *Post* continued: "Delegates from the other nations were unanimous in their opposition to doubling the levels) and voted to adopt new levels ⅓ to ¹⁄₁₀ as high." [76] A member of a special subcommittee set up by the NCRP to come up with an acceptable substitute to the ICRP's recommendations was quoted as saying 'the nation's security may demand the exposure of people to higher levels of radiation than those just established by the international body.'" [77]

The *Washington Post* was charged with journalistic irresponsibility during this period. In turn, the NCRP was accused of suppressing the higher standards set by the ICRP. The controversy climaxed with the appearance of *Post* reporter Edward Garmarekian before the Special Subcommittee on Radiation. One Committee member had characterized Garmarekian's writing as "irresponsible reporting of facts which were accurate but which put into disproportion the whole story." [78] In a prepared statement, Garmarekian substantiated his news stories by quoting Atomic Energy Commission figures.[79] After Garmarekian's presentation, Representative Holifield assured him there was no intent to reflect upon his sincerity and capacity as a reporter.[80]

The preceding events show aspects of the radiation conflict over public health and safety. But the very nature of a scientific controversy causes debilitating effects. Incisive statements are dulled by a flurry of opposing remarks. Interpretative mathematical formulae are balanced with equally impressive statistics and public understanding is lost in the fine print of voluminous published hearings.

Thus the hearings over radioactive waste disposal provide the public with an invaluable view of the very vacuum existing in radiological health and the necessity for arbitrary decision-making.

The storm broke on May 5, 1959 when Senator Ralph W. Yarborough (D.-Tex.) attacked a plan to dump radioactive wastes into the Gulf of Mexico.[81] On June 21, 1959, the Committee on Oceanography of the National Academy of Sciences released a study showing 28 sites selected for dumping. The sites included the East and West Coasts plus the Gulf of Mexico.[82] Yarborough pursued his attack. At a congressional hearing, he asked Lauriston S. Taylor if radioactive wastes should be disposed of in the Gulf Stream where the water was in motion:

"Taylor, a National Bureau of Standards radiation expert who also heads the quasi-official National Committe on Radiation Protection, said he didn't know. If there is any radiation, 'the mess will get dragged out,' he ventured.

"'It looks like one is about to brew,' Yarborough said.

"Taylor pointed out that the Bureau of Standards itself was disposing of low-level radioactive wastes by placing them in containers and dropping them in the ocean. He didn't know how long it would take the containers to rot, but said it would be a 'long time.'" [83]

The "long time" was admitted by Harold L. Price, Director of the Division of Licensing and Regulation of the AEC, to be only ten years.[84]

Governmental indecision was shown by Senator John O. Pastore (D.-R.I.). (The oceanography report had located two sites 10 miles off the coast of Rhode Island and another 22 miles from Boston. The depths of the Rhode Island sites were only 48 to 126 feet.) Pastore commented during the cross-examination of an AEC witness:

"In one day we are told that you choose three sites in all probability based upon the testimony of scientists that they would be safe enough. Then when the furor stars, we are told that two of these sites are being abandoned because they will interfere with the fishing activities in the area.

"Is that no reason for the people to be disturbed? Is that not cause for apprehensions? I say this if we are going to have the confidence of the people and if we are going to educate the public on the safety involved, I think we ought to do it a little more judiciously." [85]

[76] Ibid.
[77] Ibid.
[78] *Washington Post*, May 8, 1959.
[79] "Fallout from Nuclear Weapons Test," hearings before the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, pp. 1560–1563.
[80] Ibid., p. 1563.
[81] *Washington Post*, May 16, 1959.
[82] *Washington Post*, July 30, 1959.
[83] *Washington Post*, May 16, 1959.
[84] *Washington Post*, July 30, 1959.
[85] "Industrial Radioactive Waste Disposal," hearings before the Special Subcommittee on Radiation of the Joint Committee on Atomic Energy, Congress of the United States, 86th Cong., 1st Sess., July 29, 1959, Vol. 5, p. 3106.

Pastore told the press that the oceanography report made it appear that the sites were close to shore because it would cost more to take the wastes farther out. "Economics rather than safety looked like the more important factor." [56] He added that the selection of an underwater dump now being used for unexploded bombs as a dump for radioactive wastes also made it appear that safety was secondary.[57]

New England won its case against the dumping sites. Price announced that wastes would continue to be dumped only in waters off the continental shelf where the depth is more than 6,000 feet.[58] The Gulf Coast didn't fare as well.

The Atomic Energy Commission pressed ahead for the development of the Gulf sites. The Commission adopted an intransigent position on the issue. A battle was literally joined between the AEC and the affected southern states.

Appeals from seventeen congressional and senatorial figures were to no avail. Lyndon B. Johnson, then Majority Leader of the U.S. Senate, discussed the matter twenty-two times with the AEC.[59] Nothing moved the Commission. Finally, Johnson formally brought his state into the controversy. On September 23, 1950, Mr. W. B. McCool, Secretary of the Commission, received the following "Enclosed please find exceptions to the Intermediate Decision by the Intervenor, the State of Texas." [60] The AEC maintained its intransigent position.

An indignant Lyndon B. Johnson moved outside of the formal channels of power to block the Commission. He asked the Mexican Ambassador to the United States to file a diplomatic protest citing the dangers. A diplomtaic note was delivered. Communications followed between the Department of State and the AEC. On January 11, 1960, Legal Counsel of the Commission recommended:

". . . the Commission is required to consider the foreign relations aspects of a proposed by-product material license when raised in an appropriate proceeding, that the Commission should accept as conclusive the Department of State determination that issuance of the proposed license would have 'seriously harmful effects' on our foreign relations; and that with respect to this aspect of the matter, the Commission should conclude that issuance of the license at this time would adversely affect the common defense and security of the United States. Accordingly the staff recommends that the Commission deny the application at this time. . . ." [61]

It took the force of an international protest to stop the Atomic Energy Commission.

IX

The National Advisory Committee on Radiation (NACOR) began its analysis of adequate radiation standards on March 13, 1958. Surprisingly, Dr. Russell H. Morgan did not find difficulty in developing a consensus among its members. He did find difficulty in having the NACOR recommendations accepted by the White House.

NACOR finished its study by November, 1958. The entire Committee including Lauriston S. Taylor signed the draft report which was submitted to Surgeon General Burney. The Surgeon General approved the conclusions and fowarded the draft report to the White House.

President Eisenhower gave Dr. James R. Killian, Jr., Special Assistant for Science and Technology, the task of mediating any diverse governmental views on the NACOR study. The Special Assistant was considered the President's right-hand man in interpreting and mediating between disagreeing scientific parties. Killian had come to bear the brunt of a common Government habit—if you can't resolve a problem at your level, toss it upstairs for a decision.[62]

Killian was an effective counsel. He understood that there were certain kinds of technical questions to which scientists of equivalent objectivity, competence and complete integrity would disagree. He sought to state the facts and list the possible alternative interprtations. Numerous major accomplishments were

[56] *Washington Post*, July 30, 1959.

[57] Ibid.

[58] Ibid.

[59] AEC Docket 27-9: *In the Matter of Industrial Disposal Corporation.*

[60] Letter to Mr. W. B. McCool, Secretary of the Atomic Enegry Commission from Mr. Joe R. Carroll, Assistant Attorney General of Texas, Sept. 23, 1959.

[61] Docket No. 27-9: In the Matter of Industrial Waste Disposal Corporation; Brief on Behalf of the AEC Staff, Jan. 11, 1960, p. 15.

[62] William D. Carey, "Budgeting for Science: Presidential Responsibility," *The Annals of the American Academy of Political and Social Science*, Vol. 327 (Jan. 1960), p. 81–83.

credited to him. He admitted to President Eisenhower nuclear data which showed that an atomic test inspection system was probably feasible against the earlier advise of Teller and Strauss. He was instrumental in blocking a nuclear-powered plane and pressing for construction of more powerful atom smashers.[63]

What finally happened to the NACOR draft report is indicative of the pressures brought on the Office of the Special Assistant for Science and Technology when the chips were down.

No memoranda can be cited showing views expressed or pressures brought by governmental interests. But the role of the Atomic Energy Commission, the Department of Defense and Lauriston S. Taylor's NORP through the National Bureau of Standards can be weighed by analyzing the draft version against the final report issued.[64]

The draft was openly critical of the AEC on several matters. The report stated:

"The dual role of the AEC in the promotion and development of atomic energy on the one hand and its regulation of radiation on the other is an interesting one. Generally the vesting of both promotional and regulating functions in the same agency is unwise and may be expected to present a number of serious difficulties. Foremost among these is the possibility that the agency in its zeal to carry out its promotional activity may lose sight of its responsibilities in operational safety."

The final March report toned down the criticism. It stated:

"The dual role of a single governmental agency in the promotion and development of atomic energy on the one hand and its regulation of radiation safety on the other is an interesting one. Generally, such an arrangement is unwise and may be expected to create difficulty." [65]

The draft continued its AEC criticism with a discussion of nuclear reactor installations:

"During its lifetime, the Atomic Energy Commission on a number of occasions had indeed been accused of subordinating radiation safety to economic advantage when several of its nuclear reactor installations have been planned. In the case of a power reactor, constructed at Sandusky, Ohio, for example, considerable concern was expressed by a number of individuals and groups over the establishment of such a large reactor in the center of a densely populated area.

"Whether these criticisms were well-founded or not, it is noteworth that the dual responsibility of the Atomic Energy Commission has proved embarrassing and it may be expected to prove increasingly so in the near future."

The Marsh report was rewritten to say that "the AEC . . . has been criticized for seemingly subordinating radiation safety in the interest of economy. . . ." [66] Also, that "a number of individuals and groups have expressed concern over the establishment of large reactors not far from densley populated areas." [67] The March report concluded this excerpt by saying "it is noetworthy that the dual responsibility of the Commission has been the cause of not inconsiderable misunderstanding." [68]

The draft then turned to the AEC's regulatory function:

"As time has passed it has been interesting to observe that the AEC has not always been as certain of its regulatory functions as the law would imply. Although Congress specifically decreed that the Commission was authorized to 'establish by regulation or order such standards and instructions to govern the possession and use of fissionable and by-product materials as the Commission may deem necessary or desirable to protect health or to minimize danger from explosions and other hazards to life or property,' the AEC has never exercised these powers fully by determining standards independently or promulgating regulations or orders fixing maximum levels of exposure. Instead it has decided apparently that it would be in an untenable position as a formulator of regulations

[63] William H. Stringer, "Killian Assessed as Scientific Aid," *Christian Science Monitor*, May 29, 1959.

[64] The draft report through an anonymous source. The draft will not be footnoted since it was never published.

[65] "The Control of Radiation Hazards in the United States," A Report to the Surgeon General, U.S. Public Health Service, Prepared by the National Advisory Committee on Radiation (March, 1959) p. 9.

[66] Ibid.

[67] Ibid.

[68] Ibid.

applying to its promotional activities and hence historically has been guided largely by the recommendations of the National Committee on Radiation Protection, a private scientific organization, for the development of its standards"

The March report significantly omitted whole passages:

"When the Atomic Energy Act of 1954 was written, regulation of radiation protection was continued as a prime responsibility of the Commission in the atomic energy field. However, such regulation became immediately more complex and difficult because private enterprise was encouraged to take a vigorous role in the development of nuclear science." [99]

The NCRPs' role in deciding radiation standards was then analyzed:

"Much of the responsibility for the evaluation of radiation data and subsequent preparation of safety standards in the United States has been borne by the National Committee on Radiation Protection, a private, quasi-official group. . . ."

"The National Committee on Radiation Protection, in its Handbook 61, has proposed a comprehensive operational code for the guidance of federal, state and local authorities in the development of their radiation control programs. This code, however, has the rather serious weakness that the NCRP is an unofficial organization *without direct responsibility for any operational safety program*," (editorial *emphasis*)

"If state and local governments are to have optional guidance in the development of their specific regulatory codes, these governments must not only be able to turn to some national body having scientific competence and possessing a comprehensive viewpoint in radiation safety but *to a national body with an official position prepared to assume full responsibility for its recommendations*." (editorial emphasis)

Very disturbingly, the March report turned full circle and said:

"Much of the responsibility for the evaluation of radiation data and the subsequent preparation of recommendations which may be used as guides by regular agencies in the development of their operational protection standards, has been done in the U.S. by the NCRP, a private quasi-official group of internationally known American and Canadian scientists who are modestly supported in their work by the Department of Commerce. The organization deserves great praise for the untiring effort it has given on behalf of the nation for many years.

"From time to time, a number of individuals and groups have suggested that the NCRP should be made a component of some specific governmental agency. They believe that, under these circumstances, the Committee would gain stature and its recommendations would benefit from the more official status given them. The National Advisory Committee on Radiation, however, believes that there is much merit in the independent position which the NCRP enjoys. In such a climate, the actions of the NCRP have been singularly forthright and decisive and it is felt that it would be unfortunate if these characteristics were changed." [100]

The first round had gone to the proponents of the threshold concept and their instrumentality for decision-making, the NCRP. Missing from the March report are the extended appendices which described the efforts of cumulative dose exposure. Genetic and aging affects were discussed along with the neoplastic influence on man. Concerning the neoplastic effects, Appendix A commented: "the probability that leukemia will follow a radiation exposure seems to be directly proportional to dose and there appears to be no threshold."

Budgetary recommendations received short shrift. The draft proposed a four-part program encompassing radiation standards, education and training, state services and regional and national control. The program totaled $44 million per annum. NACOR admitted that the cost was high. "However, the present situation calls for boldness and decisiveness of action."

The boldness failed to be reflected in the March version. It anticipated the cost of a comprehensive program of radiation control reaching the level of approximately 50 million dollars over a period of five years. The report recommended that the program be developed gradually, "perhaps at a level of aproximately $2,500,000" in the first fiscal year of operation. [101]

X

The NACOR report was released to the public on March 26, 1959 at a press conference called by Surgeon General Burney. The newspapers immediately

[99] Ibid., p. 10.
[100] Ibid. p. 7.
[101] Ibid., p. 19.

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picked up the one significant recommendation left in the final version. *The New York Times* reported, "A government advisory committee recommended today that 'ultimate authority' in protecting the public from radiation be transferred immediately from the Atomic Energy Commission to the Public Health Service.[102] Richard Dudman in a March 28th dispatch to the *St. Louis Post-Dispatch* characterized the report as the "first overt blow" in the "behind-the-scenes bureaucratic struggle over supervision of the nation's radiological health." [103]

John A. McCone, Chairman of the AEC, had been readying his agency for the struggle over supervision of radiation standards. On March 25th, the day before the official issuance of the NACOR study, he released a statement detailing the vast monies and resources spent on problems associated with radiation standards and protection by the AEC.[104]

To confuse the impending conflict, he called for a government-wide review beginning with a conference in the latter part of May.[105]

Senator Lister Hill, a patron of the Public Health Service, stepped into the growing void by introducing legislation assigning overall radiation safety responsibility to PHS. President Eisenhower avoided internecine conflict by issuing Executive Order No. 10831 establishing a Federal Radiation Council. The Executive Order signed on August 14, 1959 created a Council of the Secretary of Defense, the Secretary of Commerce, the Secretary of Health, Education and Welfare, and the Chairman of the Atomic Energy Commission. Section 2 specifically stated:

"The Council shall advise the President with respect to radiation matters directly or indirectly affecting health, including matters pertinent to the general guidance of executive agencies by the President with respect to the development by such agencies of criteria for the protection of humans against ionizing radiation applicable to the affairs of the respective agencies. The Council shall take steps designed to further the inter-agency coordination of measures for protecting humans against ionizing radiation." [106]

The NCRP and the AEC had thwarted the efforts of PHS to gain control of radiological safety. Now they moved to dominate the Federal Radiation Council (FRC).

On May 19, 1959, Senator Anderson introduced S. 1987, a bill to amend the Atomic Energy Act of 1954, as amended, with respect to cooperation with the states. The bill was referred to the JCAE.[107] On August 18, 1959, four days after the signing of Executive Order 10831 creating the FRC, Senator Anderson introduced S. 2568 as a "clean bill" superseding S. 1987.[108]

Senate Report 870 on S. 2568 stated that the bill contained the principal provisions of its predecessor, S. 1987.[109] But a significant subsection had been added formalizing the establishment of the Federal Radiation Council. In the introduction of S. 2568, subsection (h) read:

"There is hereby established a Federal Radiation Council, consisting of the Surgeon General, the Chairman of the Atomic Energy Commission, the Secretary of Defense, the Secretary of Commerce, the Secretary of Labor, the Chairman of the National Academy of Sciences, the Chairman of the National Committee on Radiation Protection, or their designees, and two members appointed by the President. One such appointed member shall be a qualified expert in the field of biology and medicine and one in the field of health physics, and either may be an employee of a private, State, or local agency concerned with radiation hazards and standards." [110]

The original S. 2568 with subsection (h) designed to expand the FRC and give the NCRP leverage never left Committee. An amended S. 2568 was reported out on September 1, 1959.[111]

In discussing the amendments to S. 2568 before the Senate, Senator Anderson admitted that "the Director of the Bureau of the Budget and the Secretary of Health, Education and Welfare originally objected to the provisions concerning

[102] *New York Times*, March 27, 1959.
[103] *St. Louis Post-Dispatch*, March 29, 1959.
[104] *New York Times*, March 25, 1959.
[105] Ibid.
[106] *Presidential Executive Order No. 10831*, August 14, 1959, Section 2.
[107] *Atomic Energy Legislation Through 88th Congress, 1st Session*, Joint Committee on Atomic Energy, Congress of the United States (December 1963) p. 202.
[108] Ibid.
[109] *Senate Report 870*, 86th Congress, 1st Session, p. 3.
[110] S. 2568, 86th Congress, 1st Session, p. 6, subsection (h), lines 10–21.
[111] *Atomic Energy Legislation Through 88th Congress, 1st Session*, p. 202.

the Council." He continued, "However, after further revisions, the Joint Committee was informed that the present provisions in the bill meet with no objections from the Director of the Bureau of the Budget or the Secretary of Health, Education and Welfare."[112]

The revision of S. 2568, subsection (h) was:

"There is hereby established a Federal Radiation Council, consisting of the Secretary of Health, Education and Welfare, the Chairman of the Atomic Energy Commission, the Secretary of Defense, the Secretary of Commerce, the Secretary of Labor, or their designees, and such other members as shall be appointed by the President. The Council ***shall consult*** qualified scientists and experts in radiation matters, including the President of the National Academy of Sciences, the Chairman of the National Committee on Radiation Protection and Measurement and qualified experts in the field of biology and medicine and in the field of health physics." (editorial emphasis)[113]

Subsection 4 of Presidential Executive Order No. 10831 specifically states: "For the purpose of effectuating this order, each executive agency representative on the Federal Radiation Council shall furnish necessary assistance to the Council."

Needed cooperation and assistance were not forthcoming from the Department of Defense (DOD) and AEC. Defense and AEC refused to participate in the Council's meetings. Defense had long sought exemption from maintaining any standards established on health grounds if, in their judgment, it was justified by military requirements.

In hearings before the JCAE, DOD had pleaded its case against being subjected to the health and safety standards under section 161 of the Atomic Energy Act:

"Conceivably, health and safety standards and instructions which AEC might promulgate under section 161 could make it impossible without violating such standards and instructions, for DOD effectively to put transferred weapons or special nuclear material to that use which the President has indicated he deems necessary in the interest of national defense.

"In other situations, compliance by the DOD with the AEC-promulgated standards and instructions could be accomplished consistent with the use deemed necessary by the President, but such compliance would be more costly and burdensome than compliance with standards considered adequate by the DOD."[114]

Congress failed to approve this point of view but the Federal Radiation Council opened a convenient loophole through which DOD and AEC received exemptions.

On May 13, 1960, President Eisenhower approved the first set of guidance recommendations by the FRC. It permitted each agency of government to establish its own standards. The language under which this was possible read as follows:

". . . the guides may be exceeded only after the Federal agency having jurisdiction over the matter has carefully considered the reason for doing so in the light of the recommendations in this paper."[115]

The DOD and AEC started to attend FRC sessions after the May 13, 1960 memo. Thus the FRC achieved the very opposite of the recommendations of the National Advisory Committee on Radiation. Instead of placing responsibility in a single agency of the Federal Government, it opened the door wide for each Federal agency to establish its own standards.

XII

The change in the nation's administration in January 1961, opened another chapter in the struggle over radiation safety responsibilities.

The Secretary of Health, Education and Welfare requested the Surgeon General to reconstitute NACOR for the purpose of reexamining its 1959 report.

[112] *Congressional Record*, September 11, 1959 (Daily Edition); p. 19048.
[113] *Senate Report 870*, p. 1.
[114] "Amending the Atomic Energy Act and Authorization of Stanford Accelerator Project." Hearing before the Joint Committee on Atomic Energy, Congress of the United States, 86th Cong., 1st Sess., August 26, 1959, p. 7.
[115] Memorandum to the President from the Federal Radiation Council, ***Federal Register***, May 18, 1960.

The Secretary specifically wanted an appraisal of regulatory programs and possible recommendations for the improvement of radiation safety.

NACOR met and their final draft was completed by April 1961. Again, the report was submitted to the White House for final clearance. Dr. Jerome Wiesner, Special Assistant for Science and Technology, inherited the unenviable role that Dr. Killian was forced to play. As mediator between differing governmetal views, Wiesner was unable to keep the integrity of the second NACOR draft report.

Ironically, Dr. Wiesner had the added assistance of the newly formed Office of Science and Technology (OST). As Special Assistant, Dr. Wiesner also held the Directorship of OST. OST was created to partially relieve the Special Assistant of the burden of coordinating scientific policy in the Executive Office of the President.

The combined efforts of Wiesner and OST brought forth a final NACOR report which differed materially from the submitted draft. The material difference was not due to the common disagreements over interpretative passages but to the entrenched dominant viewpointis of the AEC and DOD. Wiesner's role as mediator disappeared under the pressures of the dominant agencies. OST failed to coordinate diverse governmental views but instead served as a convenient vehicle for the positions of the AEC and DOD.

Several selected passages from the NACOR draft report follow. They were significantly omitted from the May 1962 report.[116] The passages put the problem of radiation safety and standards today in perspective.

"At the present time there is no single policy-making focus for radiation control in the federal government. Major responsibility for this function has been assigned by executive order and by law to the Federal Radiation Council. Unfortunately, however, the Council has no direct working relationship with those federal agencies concerned with the application of policy to specific problems."

". . . the Council does not, within its present framework, provide the firm anchor in decision-making processes so necessary for a sound program of radiation control."

This lack of firm decision-making and direct working relationships between the FRC and affected agencies was vividly illustrated by the events of 1962.

In May 1960, the FRC produced its first report and recommendations to the President. This report provided the general philosophy now underlying radiation protection programs of federal agencies. It introduced the concept of "Radiation Protection Guides" (RPG) and provided the first series of such guides for individuals who are exposed to radiation through their occupations.

By the time the Council issued its first report, the assumption of a radiation threshold had been severely questioned. Caught between the two opposing schools of linear v. threshold theories, the Council took the position that there was insufficient evidence on which to base a conclusion as to the existence or nonexistence of a threshold. "Therefore, the establishment of radiation protection guides, particularly for the whole population, should take into account the possibility of damage even though it may be small, down to the lowest levels of exposure."[117]

The Federal Radiation Council published a second report in September, 1961. It dealt with radioactive materials deposited in the body as a result of their occurrence in the environment and included the following major topics: (1) Radiation Protection Guides for certain organs of individuals in the general population as well as averages for exposed population groups; (2) guidance on general principles of control applicable to all radionuclides occurring in the environment; and (3) specific guidance in connection with population exposures to radium-226, iodine-131, strontium-90, and strontium-89.[118]

Particular attention shall now be paid to the RPG for iodine-131. For iodine-131, the Council weighted the evidence available particularly with respect to children's thyroid sensitivity and recommended an RPG for the thyroid gland one-half the value previously used.[119]

This guide, however, applies to the fairly long period of one year. In order to maintain radiation doses as far below the guide as possible, the Council

[116] "Radioactive Contamination of the Environment—Public Health Action," A Report to the Surgeon General prepared by the National Advisory Committee on Radiation (May 1962).
[117] Federal Radiation Council. *Staff Report #1*, May, 1960.
[118] Federal Radiation Council. *Staff Report #2*, September, 1961.
[119] Ibid.

provided daily radiation levels which if continued, would reach the value of RPG in a year. The Council also issued a graded scale of actions designed to limit the uptake of the radioactive material. Daily radiation levels in micro-microcuries (uuc.) per liter (approximately a quart) of milk were divided into three ranges of increasing concentration with actions of increasing urgency specified for each level. For iodine, the Council estimated that an average daily intake of 80 uuc. of iodine-131 would, within a year, reach the RPG recommended limit of 0.5 rem per year. The value of 80 uuc. per day was later rounded off to 100 uuc. per day and the level of 100 was taken as the lowest point of Range III.[130]

The following table shows the graded scales of action related to the intake of radiation under the three ranges for iodine-131.

DAILY LEVELS FOR SURVEILLANCE AND CONTROL

Range I.—0–10 uuc: Only action required is adequate surveillance.

Range II.—10–100 uuc: *Surveillance* adequate to provide estimates of variation in average daily intake in time and location. Detection of sharply rising trends very important.

Control such that expected average exposures will not exceed upper value of Range II, with due regard for the most sensitive population elements.

Range III.—100–1000 uuc. (intakes within this range would be presumed to result in exposures exceeding the RPG if continued for a sufficient period of time): *Surveillance* as in Range II adequate to give prompt and reliable information concerning effectiveness of control actions.

Control actions designed to reduce levels to Range II or lower, and to provide stability at lower levels should be instituted if necessary considering relation to RPG. Sharply rising trends ***would suggest strong and prompt action.*** (editorial emphasis)[131]

In releasing the Radiation Protection Guides, the FRC stated that the Guides applied to "normal peacetime conditions."[132] Also, Report #1 of the Federal Radiation Council said "in establishing radiation protection standards, the balancing or risk and benefit is a decision involving medical, social, economic, political, and other factors."[133] Inclusion of "political and other factors" would seem to imply that the benefits of nuclear weapons testing are included, but it is not explicitly stated.

XIII

The Federal Radiation Council called for strong and prompt action with continuing radiation levels in Range III. The Council stated:

"Intakes within this range would be presumed to result in exposure exceeding the RPG if continued for a sufficient period of time. However, transient rates of intake within this range could occur without the population group exceeding the RPG if the circumstances were such that the annual average intake fell within Range II or lower. Therefore, any intake within this range must be evaluated from the point of view of the RPG and, if necessary, appropriate positive control measures instituted. . . ."[134]

The scientific community realizes that fallout from nuclear weapons testing is not a process of an even spreading throughout the country but occurs in low concentrations in some areas and in high concentrations in others. Therefore when certain major cities started reporting Range III intakes in September 1961, the Public Health Service did not show alarm since the national daily average did not exceed the stated national RPG.

Triggered by the resumption of nuclear testing by the U.S.S.R. in September 1961, cities increasingly reported Range III levels. Examples of daily peaks of iodine-131 in unc. per quart of milk in 1961 were as follows: Minneapolis, 340 in October; Palmer, Alaska, 330 in October; Des Moines, 290 in October; Oklahoma City, 250 in October; Omaha, 250 in October; Pascagoula, Mississippi, 200 in September.[135]

In St. Louis, the contamination of the milk supply reached a peak of 500 uuc. per quart on September 27, 1961. Milk levels of iodine-131 remained in Range III

[130] Ibid.
[131] Robert H. Wurtz, "The Iodine Story," *Nuclear Information* (September 1962) p. 6.
[132] Ibid., p. 3.
[133] *Staff Report #1.*
[134] *Staff Report #2.*
[135] Wurtz, p. 4.

through October and most of November.[136] In October, the Greater St. Louis Citizens' Committee for Nuclear Information (CNI) wrote the Surgeon General for necessary preventive measures for Range III intakes.

RADIATION HEALTH STANDARDS

No specific reply was forthcoming, but on November 24 the Public Health Service issued a press release which read in part ". . . It should be emphasized that the quantities of iodine-131 that have accumulated in milk to date do not exceed the FRC guidelines for yearly consumption under normal peacetime conditions."

The U.S. resumed testing in May, 1962. Sharply rising trends in Range III were again observed in several cities. CNI sent another letter to PHS urging countermeasures in May. CNI pointed out in its letter to Surgeon General Terry that failure to take preventive measures was in conflict with the recommendations of the Federal Radiation Council. Apparently, the PHS had adopted the policy that a particular rise of iodine-131 into Range III, being transient, was to be treated as an individual event. But according to the CNI letter this approach was not a valid interpretation of the FRC's requirements. While the FRC stated that "transient rates of intake in Range III could occur without the population group exceeding the RPG . . .' " this was time only " '*if the circumstances were such that the annual average intake fell within Range II or lower.*' "[137] (editorial emphasis).

CNI's letter went on to point out that the cumulative dosage from tests since September, 1961 was already ⅔ of the RPG in Des Moines and Minneapolis and more than ½ of the RPG in St. Louis, Wichita and Kansas City.[138]

CNI continued that if no preventive action was taken and if testing by the U.S. and U.S.S.R. continued unabated that it was more than likely that the RPG would be exceeded by fallout during the 12 months following September, 1961.

The letter to the Surgeon General concluded:

" 'It should be equally clear that, when as a result of the cumulative dosage from a series of transient iodine-131 peaks, it is discovered that the RPG has been exceeded it will be too late for any remedial action—for this can only be taken during the time when iodine-131 is present in the environment' "[139] (ed. note: One half of the radioactivity of iodine-131 decays every 8 days).

The impasse between concerned public health groups and the PHS was broken in mid-July of 1962. Range III intakes in Utah forced the issue.

The radioactive iodine content of Salt Lake City milk climber to over 2,000 uuc. per quart by mid-July. Following atmospheric tests in Nevada, iodine–131 levels in Salt Lake City reached into Range III on July 13, attaining a peak of 1,660 uuc. per quart on July 20 and another of 2,050 on July 25 in pooled milk. A concentration of 8,510 uuc. per quart was found in the milk from one herd alone. Consumption of such milk for only five days would have thyroid exposure exceeding the RPG for an entire year. Levels also rose to 500 uuc. per quart on July 17 in Ogden, Utah, and to 200 uuc. per quart on July 13 in Logan, Utah.[140]

The Public Health Service could not ignore the Utah situation. For the very first time, the PHS publicly recognized the need to put active countermeasures into effect. Working in conjunction with the Utah State Department of Health, they devised preventive measures: feeding cattle on dry stored fodder to avoid iodine intake from contamined pastures, shipping milk with high iodine content to processing plants, bringing in milk from adjacent areas not contaiminated by heavy fallout.[141] Action followed in other states with and without the assistance of PHS.

At the conclusion of the Utah milk crisis, Dr. Donald R. Chadwick, Chief of the Radiological Health Division of PHS, stated "If action elsewhere similar to the Uath measures should be indicated such action will be taken and full information given to the public."[142]

Dr. Chadwick's statement was commendable but he had failed to foresee the shifting of standards by the FRC under the pressures of nuclear weapon testing.

[136] Ibid.
[137] Ibid., p. 6.
[138] Ibid.
[139] Ibid.
[140] Utah State Department of Health, Press Release—August 1, 1962.
[141] Wurtz, p. 8.
[142] Ibid., p. 9.

XIV

The delayed response of the PHS to the menace of heavy fallout did not go unnoticed by the JCAE. In June 1962, a letter was addressed to Secretary Abraham Ribicoff, then head of HEW and by virtue of his office, Chairman of the FRC, by Chet Holifield, Chairman of the Joint Committee and Melvin Price, Chairman of the Subcommittee on Research, Development and Radiation. The two Congressmen posed these questions:

1. Are the numerical values of the radiation protection guides established by the FRC the sole or principal criteria now used in evaluating when undesirable levels of radioactive nuclides from fallout have been reached?

2. If so, is this use of the present numerical values of the guides sufficient to indicate when and what action is appropriate to protect public health?

3. If not, is the development of further or supplementary criteria needed; and if so is it the responsibility of the FRC or the PHS or others to develop and implement such criteria.

No reply was received befor Ribicoff's resignation. A second letter was sent on August 16, calling attention to the previous communication and the necessity to clarify the meaning of the FRC's Radiation Protection Guides.

Secretary Celebrezze, the new head of HEW quickly replied. His answer to the first question was: " 'No, the Guides are not the sole criteria used in evaluating the significance of fallout." After pointing out that the Guides were established for the protection of radiation workers and the general public against exposures which might result during "normal peacetime operations," he defined this phrase to mean industrial uses of nuclear technology.[133]

As to fallout, his reply was as follows:

" 'As applied to fallout, the Guides can be used as an indication of when there is a need for detailed evaluation of possible exposure hazards and a need to consider whether any protective action should be taken under all the relevant circumstances.'

". . . The Guides have some relevance for making judgments but they do not and were never intended to provide the sole basis for deciding how and when to act.

". . . *radiation exposures anywhere near the Guides involve risks so slight* that counter-measures which themselves involve any slight hazard have a net adverse rather than favorable effect on the public well-being.' "[134] (editorial emphasis)

Secretary Celebrezze thus informed the JOAE that the Radiation Protection Guides were developed chiefly for industrial uses and were only obliquely relevant to fallout. This was an entirely new interpretation of the RPG when the reports of the FRC itself and statements of HEW are re-examined. For example:

"Sources of environmental contamination may result from fallout after the explosion of nuclear devices and during the use and processing of fuels for reactors. There are other sources which contribute relatively smaller amounts to environmental contamination."[135]

Also a press release from HEW dated August 17, 1962 regarding the action taken in Utah, pointing out:

"The Utah action was based upon the radiation exposure guidelines recommended by the Federal Radiation Council and accepted by the President."

Finally Secretary Celebrezze's definition of "normal peacetime operations" differed materially from the interpretation given by FRC Report #1 which specifically excluded from the application of the RPG only "exposure resulting from natural background or the purposeful exposure of patients by practitioners of the healing arts."[136]

This major reversal of position by Secretary Celebreeze, statutory head of the FRC, was shown on September 18, 1962. On that day, the newspapers carried a FRC statement signed by Anthony J. Celebrezze which stated that radiation exposure many times above the protective guide levels set two years ago "would not result in a detectable increase in the incidence of disease."[137]

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[133] Ibid.
[134] Ibid., p. 10.
[135] *Staff Report* #1. Sec. 3.10, Sec. 3.11 and 3.12 discuss fallout patterns and doses in greater detail.
[136] *Staff Report* #1, Sec. 7.12.
[137] *The New York Times*, September 18, 1962.

The *Times* affirmed that for the first time the Council took the official position that its radiation protection guideline did not apply to nuclear fallout.

This conclusion appeared to be the final contradiction to the basic principle used by the FRC in developing the RPG. For example, Report No. 1., Sec. 4.3 of the FRC stated:

"Over the past decade or two, there has been an increasing reluctance on the part of knowledgeable scientists to establish radiation protection standards on the basis of the existence of a threshold for radiation damage and on the premise that the threshold lies not too distant from the point at which impairment is detectable in an exposed individual."

XV

The indecisiveness of our government over national radiation health standards was again shown almost one year later.

In a concluding subcommittee statement on June 4, 1963, Congressman Price said:

"We have been told by Dr. Tompkins—the new acting Executive Director of the FRC that 'within the next year the Council will make some positive recommendations dealing with this problem.' It is my hope that the FRC will not require a full year to arrive at a solution to this problem. I would hope that within the next few months the Federal Radiation Council would arrive at radiation protection guides which *are* applicable to the problem of fallout."[138]

Senator William Proxmire of Wisconsin would not wait for the recommendations of Dr. Tompkins. On June 24, 1963, he introduced S. 5653, entitled the Radiation Hazards Act.

"My bill dispels the mists of confusion, fear, and mistrust which have frightened the American people."[139] His bill would have had the Surgeon General taking full responsibility of radiation safety and standards without the cross pressures which the FRC had been subjected to. It should be recalled that this was the essence of the 1959 NACOR report. Proxmire's bill failed to be referred to the JOAE but was sent to a subcommittee of the Labor and Public Welfare Committee.

On April 8, 1964, H.R. 10437 was introduced in the Senate: A bill to make the National Committee on Radiation Protection and Measurement (NCRP), a statutory body of the United States. The bill passed the House on April 6, 1964. It is now awaiting Senate action.[140]

S. 1764 languishes in committee. Such are the exigencies of modern-day government.

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[138] "Fallout, Radiation Standards, and Contamination," hearings before the Subcommittee on Radiation of the Joint Committee on Atomic Energy, Congress of the United States, 88th Cong., 1st Sess., June 3, 4, 6, 1962, Part I, p. 337.
[139] Press Release—Office of U.S. Senator William Proxmire, June 24, 1963.
[140] H.R. 10437, 88th Congress, 2nd Sess.

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(Whereupon, at 11:45 a.m. the committee adjourned, to reconvene at the call of the Chair.)

APPENDIX I

Supplementary Material Submitted by Dr. Gofman and Dr. Tamplin

Contents:

(Mr. Tamplin subsequently submitted the following communications and enclosures:)

UNIVERSITY OF CALIFORNIA,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., November 24, 1969.

Senator EDMUND S. MUSKIE,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: It was indeed a privilege for Dr. John Gofman and me to testify before the Subcommittee on Air and Water Pollution. It would appear that the establishment of radiation pollution guidelines involves risk vs benefit decision that should more properly be made by a much larger segment of society than a Commission which is dedicated to the promotion of a technology.

Dr. Kalkstein, who testified before us, mentioned Dr. Sternglass who published a report in the *Bulletin of the Atomic Scientists* and in *Esquire* in which he claimed that fallout from weapons tests had been responsible for 400,000 excess infant deaths in the United States. I have written a refutation to the Sternglass article which will appear in the December, 1969 issue of the *Bulletin of the Atomic Scientists*. I left a copy of this article with Dr. Grundy after the hearings.

The purpose of this letter is to indicate to you that Dr. Sternglass was both right and wrong. He was correct to the extent that there was an excess of 400,000 infant deaths in the United States but he was wrong in concluding that these resulted from fallout radiation. The inescapable conclusion is that these deaths were the result of poverty. The size of the number (400,000) indicates that I am discussing poverty on a grand scale. I am enclosing a copy of an article which will appear in the January 1970 issue of the *Bulletin of the Atomic Scientists* which describes both the extent and dire consequences of this poverty.

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We hear much talk about the forgotten American. I have a feeling that once he becomes aware of the consequences of this neglect, the forgotten American will insist upon being remembered.

If I can be of any assistance to you or your subcommittee please let me know.

Respectfully yours,

ARTHUR R. TAMPLIN,
Bio-Medical Division.

POVERTY IN THE UNITED STATES

(By A. R. Tamplin)

In an article entitled, "Fetal and Infant Mortality and the Environment," in the December issue of the *Bulletin of the Atomic Scientists*, I presented evidence which demonstrates that the major differences in infant and fetal mortality are based upon socio-economic conditions. The purpose of this article is to show that even in the United States, a so-called affluent society, we have poverty on a grand scale.

INFANT DEATH RATE AND LIFE SPAN

The discussion of the physiological basis for the fetal and infant death rates presented in the article mentioned above is in substantial agreement with a theory of aging advanced some years ago by Hardin Jones of the University of California in Berkeley. In this theory Dr. Jones proposed that the death rate of a population at any age was dependent upon the physiological injury that the population has accumulated up to that age. He argued that one could change the rate of accumulation of new injury and thereby alter the subsequent death rate in future years. *But the data indicated that the physiological injury accumulated during childhood was far more important than that accumulated during adult life.* The death rate during adult life is thus primarily determined by the injury accumulated during the period of development and maturation. The earlier in life the physiological injury is accumulated, the more significant the injury. Now, quite obviously, a population that experiences a higher death rate at all ages will have a shorter average life span. For a human population, a doubling of the death rate corresponds to a reduction of 8 years in the average life span. Increasing the death rate by threefold reduces the average *life span by some 13 years.*

Jones was able to demonstrate that throughout the world, there was roughly a 1 : 1 correspondence between infant mortality and subsequent adult death rates. This relationship is also seen in the white-non-white data of the United States. Thus the infant death rate of a population is not an isolated statistic. It defines the physiological competence of the young adults in the population and consequently, the average life span of the population. The Negro infant mortality in this country is twice that of the white population and the average life span of the Negro population is 8 years less than that of the white population. The difference between the Negro death rate and the upper middle class white death rate is roughly threefold. Consequently, these whites have an average life span that is 13 years longer than the Negro population.

WHAT IS POVERTY?

There is no adequate definition for poverty. It is a word of the emotional side of man. A man's definition of poverty will depend upon his individual status, his background, his ambition or drive, and his degree of immorality. Ultimately, it must be conceded that defining poverty is a moral decision. On the other hand, wealth can be defined in a rather scientific manner, to wit, wealth is optimum health. Scientifically then, poverty becomes a state of relative health.

This scientific definition of wealth and poverty is given in Fig. 1 where the infant death rate in the United States is plotted as a function of a family's yearly income based upon 1965 data. This curve is drawn on the basis of 3 points: 1) The average Negro income and infant death rate, 2) the average white income and infant death rate, and 3) an assumed present-day limit to infant mortality of 12.5 per 1000 live births plotted at $10,000. A similar curve would be obtained using the English data. The lowest income group in England has an infant death rate comparable to our Negro population. This curve demonstrates, under this scientific definition of wealth, that families with an income below $10,000 per year live in a state of relative poverty.

THE DIMENSIONS OF POVERTY

In 1965, 25 per cent of the United States population had a family income in excess of $10,000 per year. Therefore, 75 per cent of the population was in a state of relative poverty. In 1965, 50 per cent of the population (40 per cent of the white population and 75 per cent of the Negro population) had a family income of less than $6000 per year. Figure 1 indicates that incomes below this level are associated with infant death rates that are more than double that of the upper 25 per cent group. The preceding section of this article (on life span) indicates that this infant death rate is associated with more than an 8 year reduction in the average life span. The government figure for the poverty level income during this period was in the neighborhood of $3000 per year. This income is associated with a fourfold increase in infant death rate and a 16 year reduction in life span. Some 20 per cent of the population (17 per cent of the white population and 37 per cent of the Negro population) was below this governmental poverty level income. It would seem that by most definitions we have poverty in this country and have it on a grand scale. Like any other form of environmental pollution, we should seriously consider what is an acceptable level. Then, hopefully, we can exert sufficient pressure to cause our national priorities to be re-aligned so that our advances in science and technology, our money and knowledge, and our manpower can be put to work on improving the quality of life in our country.

The dimensions of the poverty problem as shown by Fig. 1 demonstrate that the solution of this problem should be given the highest national priority. But poverty is not simply a matter of money or jobs; it is a way of life. Poverty is a family income within a social, economic, and cultural milieu. It is the product of the numerous interplaying factors that represent the total society.

Thus, poverty is not an isolated problem. Its solution will involve most of the major problems that confront rural and urban America today. For example, the national statistics show that 50 per cent of the girls who were in high school in the late 50's and early 60's were destined to raise families on an income below $6000 per year. I wonder whether their education was truly relevant to this fact of life. Maybe the food industry in this country should produce, market, and advertise products such as low-cost, high-protein food supplements. The relationship between the private automobile and smog is legend. The need for an automobile is a sizeable financial drain on low-income famililties. Hence, poverty is related to the problem of air pollution and the need for adequate systems of public transportation.

What we need is a master plan for improving the quality of life in this country. It cannot be a piecemeal operation. What is done in one sector affects all others. We have the capability today to look at these large problems, to isolate the various interplaying factors, to determine the nature of the interplay, and to propose an integrated solution to the problems. We have the scientific and technological knowledge; we have the industrial capability operating within an extremely viable free enterprise system; and as the space program demonstrated, we have a genius for organization. We could improve the quality of life in this country if we made the effort.

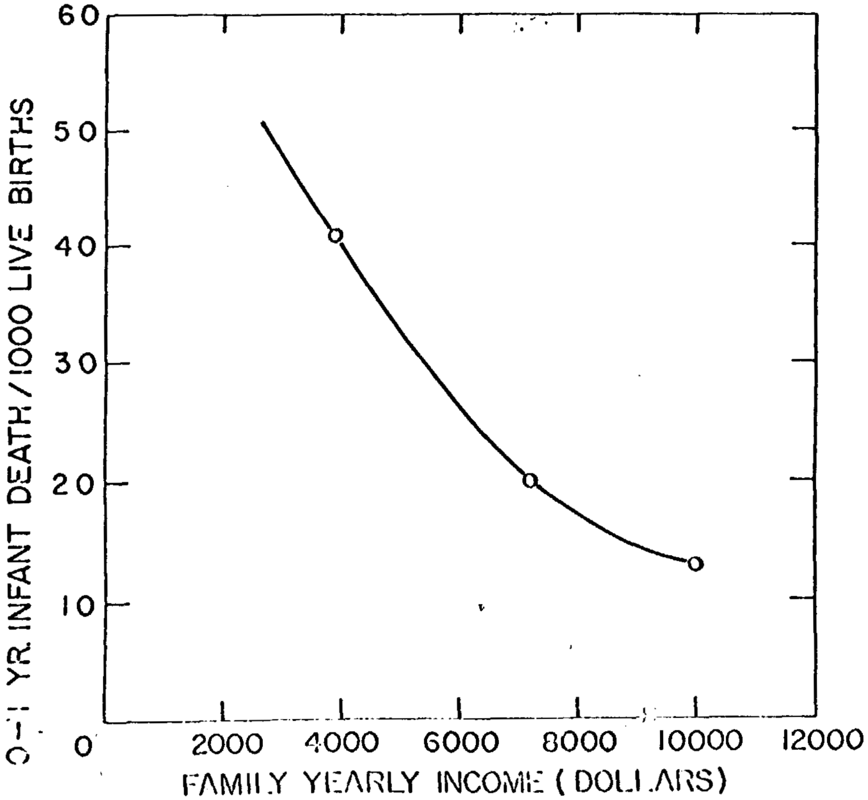

FIGURE 1.—The 0-1 yr death rate as a function of family income in 1965 is presented.

UNIVERSITY OF CALIFORNIA,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., December 1, 1969.

Hon. EDMUND S. MUSKIE,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: In our effort to keep you informed of further developments on the important subjects of your inquiry, we are enclosing two recent clippings that you will find of great interest.

You may recall, in our testimony we said that the cancer-leukemia problem was evidence enough, but that even larger hazards were going to be demonstrated. No sooner said than done. The United Nations Committee now reports that mental retardation due to radiation is a *major* hazard. This will help point out to you that our testimony is not that of "kooks and conservationist-nuts", as some of the defenders of parochial interests would have you believe.

We, as scientists, can only serve to inform you of facts. And we shall continue to provide you with every new item of evidence.

But the task of protecting the public of the USA has to rest upon the forward thinking Senators, such as you and your Committee colleagues. While parochial

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interests will certainly press upon you, it is our belief the public will appreciate deeply your efforts to keep the earth habitable by humans.

Sincerely yours,

JOHN W. GOFMAN,
ARTHUR R. TAMPLIN.

(Articles follow:)

[From Hospital Tribune Report, Nov. 17, 1969]

U.N. STUDY IN JAPAN LINKS BRAIN HARM, 50 RADS TO FETUS

UNITED NATIONS, N.Y.—There is clear evidence that radiation in doses of 50 rads or more from the second to the sixth month of prenatal life causes retardation and microcephaly, according to a report of the United Nations Scientific Committee on the Effects of Atomic Radiation.

"Evidence of the effects of lower doses during this same period of prenatal life is still extremely tenuous and does not permit exclusion of the possibility that increased incidence of the same effects may be the result of exposure in this lower range," the report warned. Available data, it also said, suggest that even low doses of radiation given to the fetus later in pregnancy may increase the incidence of tumors of the nervous system as well as other malignancies. These conclusions were based in part on studies of 1,613 children—sixteen percent of the total liveborn—who were in utero in Hiroshima and Nagasaki at the time of the atomic bombings. Although head size and mental retardation were first observed in Nagasaki in 1951, studies made at 17 and 20 years of age have just become available, the report said.

"It is remarkable," it commented, "that in both cities, all cases of mental retardation within 2 Km. from the hypocenter were born between November, 1945, and March, 1946, corresponding to exposure between the sixth and 24th week of pregnancy, with a peak frequency at 13 weeks in the proximal group and at 14 weeks in the distal one.

Incidence of retardation among these subjects at 17 years of age who were in utero and less than 1,500 M. from the hypocenter was 7.9 per cent for males and 7.5 per cent for females in Hiroshima, and 16.5 and 5 per cent, respectively, in Nagasaki.

Study of the prevalence of mental retardation according to the dose of radiation, the report said, showed that among women who received doses of 200 rads, 86 per cent of the offspring were retarded. Doses of 100–199 rads produced retardation in 9.3 per cent of offspring, and 50–99 rads in 4.6 percent. For over 500 pregnant women who were not in the cities at the time of the blast, the rate of retardation of offspring was about 1 per cent.

The report stated that mental retardation is not the only serious affect in the nervous tissue that is associated with prenatal irradiation. It cited surveys showing an "increased frequency of in utero irradiation for medical reasons among children dying of malignancies of the nervous system compared to controls." In one of the surveys, the observed excess indicated that the incidence of malignancies of the nervous system is about 40 per cent higher among irradiated children.

"No excess of these malignancies has been reported in subjects irradiated increased incidence of the same effects may be the result of exposure in this lower range," the report warned.

Available data, it also said, suggest that even low doses of radiation given to the fetus later in pregnancy may increase the incidence of tumors of the nervous system as well as other malignancies.

These conclusions were based in part on studies of 1,613 children—16 per cent in utero at Hiroshima and Nagasaki," the report said. "The number of subjects so exposed was too small, however, for increases of tumors of the nervous system to have been detected in those populations, unless the rates of induction had been much higher than the surveys previously referred to suggest."

Most of the long-lived nuclides injected into the stratosphere by earlier nuclear tests, it said, had been deposited by 1967, but "substantial fractions of total doses to which the population is committed remain to be received from present body burdens and from deposit in soil that will continue to be transferred to food-stuffs."

Present estimates, it said, indicate that roughly one-eighth of the total expected population does of strontium ^{90} had been delivered by the end of 1967. Only a small fraction of the population dose of carbon ^{14}, the radioactive half life of which is

much longer, has been delivered so far, and somewhat less than one-tenth of it will have been delivered by the year 2000. In contrast, more than half of the contribution to the dose commitment from external sources has already been delivered.

The 165-page report was the work of radiologists and other scientists who make up the United Nations Scientific Committee on the Effects of Atomic Radiation. The committee, established in 1955 by the General Assembly, consists of representatives of 15 countries. Dr. Richard H. Chamberlain, chairman of the Department of Radiology at the University of Pennsylvania, is the United States representative on the committee.

[From the Hospital Tribune—World Wide Report]

A World Meeting Warned of Danger of Nuclear Plants

HANOVER, WEST GERMANY—The problem of the possible dangers to health from atomic reactors was highlighted here at the International Convention of Civilization Diseases.

Dr. W. Herbst, of the Radiology Department, University of Freiburg, West Germany, accused the atomic energy industry of neglecting safety measures. "In spite of all the risks to man of exposure to tritium and krypton[85], German legislation still permits their emission into the atmosphere," he said.

PUBLIC NEVER INFORMED

Noting that the public has never been informed about the possible long-term risks of living close to a nuclear power station, Dr. Herbst stressed the danger to populations absorbing 0.5 rem more than normal every year during a lifetime of say 70 years. One of 500 persons would die from cancer or leukemia and more than 300 first-generation children in a population of 1,000,000 would present chromosome damage, he said, from absorbing such a dosage, which would amount to a total of 35 rem.

"In future decades we will certainly be faced with such a situation in Germany where millions of people are or will be living close to atomic reactors," Dr. Herbst went on.

Such would be the result even if the reactors operated normally. "But they can go wrong, and elsewhere they have," he declared.

He cited an accident at the Windscale reactor in Britain that put 22,000 curies of radioactive iodine into the atmosphere and resulted in the destruction of more than 1,000,000 L. of milk.

"If such an accident had occurred near a population center the long-term danger to health would have been incalculable," he commented.

Dr. M. O. Bruker, head of the internal medicine department, Ebeneger Hospital, Lemgo, West Germany, accused authorities of fostering the belief that atomic reactors could not go wrong.

"But why should reactors be different from any other types of technical plant which do go wrong?" he asked.

Dr. Bruker pointed out that one unsolved problem is how to cycle water coolant, since atomic reactors need twice as much coolant as normal power stations.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif, December 1, 1969.

HON. EDMUND S. MUSKIE,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: In the question and answer section of your Subcommittee Proceedings of November 18, 1969, you asked us if the AEC had commented upon our findings. We indicated then that they were digesting our reports. We have now received several questions and suggestions from the Atomic Energy Commission. The answers, we believe, are so important and so germane to the deliberations of your Committee that we must provide them for you as a vital supplement to the testimony we have already provided.

We shall address every one of the points raised through the AEC in the following pages, but one, in particular, is so important, we must immediately address it here.

The commentary from the AEC includes a suggestion that:

> "For example, in the case of induction of bone tumors in the radium dial painters, the conclusion should be inescapable that there is a threshold dose below which tumors do not appear."

We were shocked and dismayed to find that any scientists concerned with such matters still took seriously the results obtained through the study of persons exposed to radium and related alpha-emitting radionuclides. Indeed, such studies apparently do influence scientists concerned with standards right up to the present time. Upon searching this matter further, we failed to find a refutation of the conclusions arrived at in some quarters through the studies of radium-exposed persons. We felt it incumbent upon us to prepare for your Subcommittee, and for inclusion in the Record of these Hearings, a critical analysis of the entire set of observations concerning radium-exposed persons. This we have done, and it is available directly following this letter to you.

The summary result of this critical analysis is a demolition of the last vestige of any evidence that any "safe threshold" exists for exposure of *humans* to ionizing radiation. Since apparently many responsible scientists still do not understand the fallacies in the radium results and interpretation, it is of vital importance that the full analysis be included in these Hearings.

Following that supplementary testimony, we have also addressed ourselves to several other questions raised through the AEC. Each of these is addressed in subsequent pages, for inclusion in these Hearings.

The questions and suggestions raised by the AEC have enabled us to provide answers which immeasurably enhance our arguments *against* perpetuation of the existing Federal Radiation Council Guidelines. We had hoped for many more "refutations" from the AEC and the FRC than we have received. *Every one* received we have answered for you here. It may be, however, that potential criticisms of our work will appear elsewhere, such as in scientific journals or in other hearings. We shall watch for these carefully and answer them on the highest scientific plane as they occur.

We had hoped that some evidence refuting us would have arisen, because we would really hope our findings, which are discouraging, might be wrong for reasons not yet clear to us. On the other hand, the absence of refutation of our case against perpetuation of the FRC Guidelines may mean it is a "re ipso loquitur" case already.

Sincerely yours,

JOHN W. GOFMAN,
ARTHUR R. TAMPLIN.

Supplementary Testimony: AEC Questions and Our Answers

(John W. Gofman and Arthur R. Tamplin)

Item 1.—The AEC raised for our attention a recent article by Miller concerning the Japanese data in the Atomic Bomb Casualty Commission reports. We had, as you will note, already referenced this article in our initial testimony.

While we had already demonstrated in the initial testimony why we felt there was no significant merit in Miller's questions about lung cancer induced by radiation in Hiroshima-Nagasaki survivors, we would like to amplify here. Such amplification does not detract from our general laws on radiation induction of all forms of cancer; indeed, such amplification materially strengthens our evidence. There are two central issues raised in Dr. Miller's article. We shall take up each in turn.

(1) *Lung cancer induction by radiation in Hiroshima-Nagasaki survivors*

Dr. Miller has raised the objection that the kind of cells in the Japanese cases were different from the kind of cells in the cancers of the uranium miners. And then he goes on to question that radiation could have been responsible for the Japanese cases of lung cancer. Nothing could be further from the truth.

Dr. Miller seems to have overlooked a fundamental law of radiobiology, namely, that radiation does not produce different cancers from those which occur

spontaneously. Instead it appears to make the incidence of such cancers *higher* at an earlier age than without radiation.

Second, Dr. Miller has overlooked the real reason, in all probability, why the uranium miners have a different *kind* of cancer from the Japanese and why both are radiation-induced. Essentially all of the human evidence available indicates that radiation produces cancer in the cells it strikes, rather than by an indirect mechanism. Now, the uranium miners were exposed to radon daughters, which chemically are polonium, bismuth, and lead. In all likelihood, these radon daughters lodge in lung tissue in such a locale that the intense irradiation is produced in a group of cells different from those lining the small bronchi. And this is most certainly why the uranium miners have cancers of a cell type different from the common bronchiogenic lung cancer. So, far from arguing that radiation should be expected to produce a specific form of lung cancer no matter where the radiation strikes, what Dr. Miller should have realized is that radiation of *different sites* produces different kinds of cancers.

Additionally, the implications of the fact that uranium miners have primarily a form of lung cancer that is spontaneously much less common than the usual bronchiogenic lung cancer must be pointed out. Now, we estimated 125–250 Rads as the doubling dose for lung cancer in the uranium miners. Since the kind of cancer induced is spontaneously a less frequent kind than bronchiogenic cancer, the net result is that this means the doubling dose for lung cancer in the miners must be appreciably *lower* than 125–250 Rads we estimated. This makes the radiation effect even worse than we had estimated; however, it brings the uranium miner data into better alignment with all the other doubling doses we presented in the original testimony.

Also, since the uranium miner data suggest a different kind of cancer from the Japanese data or the British data, we can now add *one more* new kind of cancer to the already overwhelming list of human cancers induced by radiation.

(2) Breast cancer induction by radiation in Hiroshima-Nagasaki survivors

Dr. Miller, for reasons not made clear, seems unhappy that breast cancer should have been proved to be induced by radiation in survivors of Hiroshima-Nagasaki. He provides no reason or any reasoning for it, but blandly states "it may be difficult or impossible to avoid certain biases". In scientific parlance, this is what we might refer to as "reaching." If Dr. Miller had examined the Hiroshima-Nagasaki breast cancer data a little more closely, he would have realized that the data are even more damning concerning radiation than the ABCC report claimed. The ABCC data on radiation-induced breast cancer showed that the breast cancer in the heavily irradiated cases occurred some 11 years younger than in the non-irradiated or lightly irradiated cases. For this age difference, the expected *spontaneous* breast cancer rate in the younger group would be less than half of that in the older group. As a result, instead of the doubling dose for breast cancer in Japan being 100 Rads, the true value would appear to be 50 Rads or less. In our analysis we had endeavored to be as conservative as possible, so as not to overstate the case against radiation-induction of human cancer. But since the Miller criticisms were raised by the AEC, we feel compelled to point out that the case against radiation in the Hiroshima-Nagasaki breast cancer victims is not weaker than claimed in the ABCC studies, but much stronger.

Item 2.—The AEC raised the suggestion that the most appropriate way to look at radiation effects was to study the life-shortening effects in animals, which effectively includes cancerogenesis. The suggestion goes on to point out that in such studies radiation such as X-rays, γ-rays and β-rays, delivered slowly over a long period of time are approximately ⅕ as effective in shortening the life of experimental animals as is the same amount of radiation delivered in one acute dose. The AEC comment proposes that the probable mechanism is that the latent period of inducing effects is so long, with protracted radiation, that the animals don't live long enough to show the damaging effect. And the AEC comment further quotes the radium-dial painter work as showing a "threshold" probably through this same mechanism.

In the first supplement to our testimony, we treated the radium-dial painter results in extenso. We pointed out that as a result of our analysis, the evidence upon which a threshold rests simply doesn't exist anymore. It never did exist, but it hadn't been pointed out before. We can, therefore, dismiss this last issue without further comment.

The other points raised by the AEC suggestion do deserve comment.

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(1) The Life Shortening Effect in Animals Being a Good Description of Radiation Effects, Including Cancerogenesis

We are in complete agreement with the AEC comment that the life-shortening effects other than cancer should be taken into account. Indeed, in our initial testimony we pointed out that if we took life-shortening other than cancer into account, based upon experimental animal data, we would have to multiply our estimate of 16,000 premature deaths from cancer and leukemia by a factor of 4, making the number 64,000 premature deaths from all causes annually in the U.S. due to exposure of the population at the existing FRC guidelines. We are somewhat amazed at what amounts to the insistence, in effect, by an AEC criticism that we should use the pessimistic number of 64,000 instead of 16,000. We are so horrified by the contemplation of 16,000 additional deaths per year based upon *solid human data* that, even though we agree with the AEC that 64,000 deaths is a more probable number, we shall not use it here because it rests upon experimental animal rather than human data.

With respect to the second issue raised by the AEC that protraction of the radiation reduces life-shortening effects in animals by a factor of five, we have several comments. First, all the studies referred to by the AEC are in short-lived animals compared to the 70-year life span of man. An effect that is delayed in a mouse, for example, by 3 years, may indeed not get to manifest itself simply because the mouse doesn't live long enough. But for an effect delayed 3 years in a mouse, we have no way of knowing whether it will be delayed 3 years, 10 years, 20 years, or 100 years in man. The "scaling" laws from mouse to man, or rat to man, or dog to man, simply are totally unknown in this field of radiation effects. As a result, a delayed effect that *never* gets to manifest itself in a mouse *may* manifest itself *fully* in man, and hence, there may be no significant protection whatever due to protraction of radiation in the human case.

We are eager to join the AEC in praying that the ultimate results will prove that protraction of radiation does reduce the hazard of a particular dose of radiation to man. This would, indeed make the outlook for man less dismal if he receives radiation unnecessarily through ill-conceived atomic energy projects. But our problem and the problem of this Subcommittee has to do with the validity of radiation standards vis-as-vis such projects as Plowshare and atomic power, not with prayer for some favorable future discovery. In the hard world of reality, where we are all concerned with the protection of the welfare and life of the citizens of the USA, we must necessarily deal with the hard facts in our hands today, rather than some ephemeral future hoped-for silver lining in a dark sky. We, therefore, must reject the AEC suggestion to count on a factor of five protection from protraction of radiation in man.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., December 3, 1969.

Hon. EDMUND S. MUSKIE,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: Yesterday we sent off to the Subcommittee Staff the corrected transcript of our testimony plus important additional material, provided in response to some of your questions relative to the Hearings.

One item, a new publication prepared for inclusion in the Hearings is so important, we believe, that we wanted you to have a personal copy for your immediate reference. It is entitled, "Studies of Radium-Exposed Humans: The Fallacy Underlying a Major Foundation of NCRP, ICRP, and AEC Guidelines for Radiation Exposure to the Population-at-Large".

As you will see from reading it, the very pillars of *all* radiation protection standards now stand exposed as being a set, truly, of rotten pilings.

Your work, and that of your Colleagues, becomes of even greater urgency and national moment as a result of these new disclosures.

With kindest regards,

Sincerely yours,

JOHN W. GOFMAN
ARTHUR R. TAMPLIN.

[Enclosure follows:]

Studies of Radium-Exposed Humans: The Fallacy Underlying a Major "Foundation of NCRP, ICRP, and AEC Guidelines for Radiation Exposure to the Population-at-Large"

(By John W. Gofman and Arthur R. Tamplin, Division of Medical Physics (Berkeley) and Bio-Medical Research Division, Lawrence Radiation Laboratory (Livermore), University of California)

INTRODUCTION

The guidelines which specify the maximum limits of exposure of humans to ionizing radiation from peaceful uses of atomic energy represent a set of numbers having as great an impact upon the future of the human race as any set of numbers ever could. Therefore, society must demand, as an item of the very highest priority, that such guidelines be absolutely above reproach and question, for the consequences of error can even mean the deterioration of the human race on earth.

Recently we have attacked the Federal Radiation Council Guidelines for such exposure on the grounds that if everyone received the Guideline dosage, some 16,000 additional cases of cancer plus leukemia would occur each year in the United States (1) (2).

It is the purpose of this communication to demonstrate that one of the *purported major foundations* of guidelines established by the ICRP, the NCRP, and the FRC is totally without basis in fact and rests upon the overtly erroneous interpretation of some otherwise extensive careful observations on humans. We refer to the belief that a threshold (practical or absolute) was demonstrated through the studies of radium dial painters, chemists exposed to radium, and persons receiving radium or related alpha emitters medically.

The chief proponents of the belief that the data accumulated through the study of such individuals leads to a valid "threshold" below which no injury occurs is Professor Robley D. Evans of the Massachusetts Institute of Technology. Dr. Evans is to be commended for a beautiful series of investigations extending over 30 years which have greatly increased our knowledge concerning radium and its effects upon man. However, we shall develop the evidence here to prove that Dr. Evans' conclusions from his own and from other data are totally erroneous with respect to demonstrating or even suggesting a "safe threshold" of ionizing radiation.

We can best start this evaluation by a series of quotations of Professor Evans, quotations of such deep consequence as to possibly affect the future of every living human and those unborn.

Quotation 1 (Reference 3)

"The effects of skeletally deposited radium and mesothorium are of immediate relevance here. These studies have provided the permissible body burden for radium in humans. It is the only NCRP, ICRP, Atomic Energy Commission permissible dose based directly upon observations on humans, and is the pivot or reference point for the permissible burdens of plutonium and of strontium-90."

Quotation 2 (Reference 4)

"It is my conviction that there does exist an absolute threshold and a practical threshold for inhaled radon daughters, below which these nuclides are innocuous."

Quotation 3 (Reference 5)

"Thus it will be seen that the present RPG of 0.1μC Ra contains a large safety factor and would appear to be a satisfactory value even if applied to large populations."

Quotation 4 (Reference 6)

"In the present series of hearings this committee has been exposed to the conservative, oversimplified, incorrect, linear and non-threshold model of radiation carcinogenesis."

These represent four quotations of great assurance and of far-reaching implications. We shall now, through analysis of the data upon which professor Evans bases these conclusions, demonstrate that the conclusions implied in these quotations are not correct, and are in no way supported by the evidence upon which they rest.

THE EXPERIMENTAL OBSERVATIONS

This analysis will address itself to the data concerning the occurrence of cancer (carcinomas plus sarcomas) in persons carrying various measured residual body burdens of radium. Evans has presented the data for one series of cases (269 persons in all) with the occurrence of cancer in individuals in relationship to the residual radium burden (5). Hasterlik has presented an entirely separate series (264 women, some 36 years after occupational exposure to radium) with the occurrence of cancer in individuals in relation to residual radium burden (7). These data are reproduced in Table 1 (Evans data) and Table 2 (Hasterlik data). As Evans correctly pointed out, there is remarkably good agreement between the two sets of data (8). However, we must add there is remarkably good further agreement in the fact that *neither* set of data supports the conclusions drawn by Evans.

TABLE 1 (REFERENCE 5).—DATA FOR 269 CASES WHERE A PURE RADIUM EQUIVALENT (RESIDUAL BURDEN IN μC RA) WAS ESTIMATED (DIAL PAINTERS, CHEMISTS, PLUS MEDICALLY TREATED PERSONS)

Number of cases	μC Ra equivalent residual: Dose range	μC Ra equivalent residual: Median dose	Number of cancers
42	<0.001	<0.001	0
61	0.001–0.01	0.0055	0
80	0.01–0.1	0.055	0
32	0.1–1.0	0.55	3
40	1.0–10.0	5.5	14
14	10.0–100.0	55	2

TABLE 2.—(REFERENCE 7)—DATA FOR 264 WOMEN (~36 YEARS AFTER OCCUPATIONAL EXPOSURE)

Number of cases	μC Ra equivalent residual: Dose range	μC Ra equivalent residual: Median dose	Number of cancers
23	<0.001	<0.001	0
36	0.001–0.01	0.0055	0
102	0.01–0.1	0.055	0
62	0.1–1.0	0.55	3
41	>1.0 (1–10)	5.5	14

ANALYSIS OF BOTH SETS OF DATA

The hypotheses that have been set forth by Evans, exemplified in the quotations above, are:

(1) These data indicate that there exists a threshold value below which radium deposition in the skeleton does not produce cancer in humans.

(2) These data indicate that the linear model of radiation carcinogenesis is incorrect.

Let us approach both of these hypotheses, since they are closely related. At first glance, it is to be noted, in these extremely small series of humans, that none of the observed cases of cancer occurred in any of the dosage ranges below 0.1 μC Ra residual burden in either series of cases. We can admit even further that in the Evans series (Table 1), the lowest dosage where a cancer occurred is 0.6 μC, and in the Hasterlik series, the lowest dosage with cancer is 0.45 μC. But such a first glance observation does not even remotely resemble ***an analysis*** and ***does not bear at all*** upon the validity of the Evans hypotheses listed above. We must, therefore, proceed with an analysis.

(*a*) ***Analysis of the Evans data*** (Table 1)

The first step is to determine the probability of finding cancer in these subjects in relationship to dose of residual Ra burden. This can be done either using only the group of cases (1.0–10.0 μC Ra) with the largest number of cancers, since it is most reliable, or by using all the data for groups where cancers occurred (0.1–1.0, 1.0–10.0, 10.0–100.0 μC Ra). We shall do the analysis both ways, for the sake of completeness.

For the group of cases with burdens of 1.0–10.0 μC Ra there were 14 cases of cancer out of 40 total persons.

$\frac{14}{40}$ is, therefore, the probability of cancer for a median dose of 5.5 μC Ra.

So, per μC *Ra*, $\frac{14}{40\times5.5}=0.064$ is the probability of cancer.

Expressed alternatively, 6.4 cases per 100 people are found for a burden of 1 μC Ra.

Now, we can look at the three low dose ranges where no cancers were observed. The linear thesis would *expect*, for such low dosages, 6.4 cases per 100 persons per μC Ra residual burden.

The 0.01–0.1 μC Ra range

We have 80 persons in this group with a median residual burden of 0.055 μC Ra.

For 80 persons, therefore, our expectation is:

$$\left(\frac{80}{100}\right)\times(6.4)\times(0.055)=0.28 \text{ cases of cancer expected.}$$

Cancer in humans cannot occur as fractional cases. Therefore, in our group of 80 persons, occurrence can be 0 cases, 1 case, 2 cases, etc. If our expected number of cases is 0.28, then there are at least 72 chances out of 100 of *observing* 0 cases. So the probabilities are strongly in favor of observing 0 cases, which happened.

Conclusion.—The data are completely *consistent* with the linear thesis and completely *consistent* with the absence of any threshold "safe" dose in this range. The data provide nothing at all to indicate we should accept *either* of Dr. Evans hypotheses.

The 0.001–0.01 μC Ra range

We have 61 persons in this group with a median residual burden of 0.0055 μC Ra.

For 61 persons, our expectation is:

$$\left(\frac{61}{100}\right)\times(6.4)\times(0.0055)=0.021 \text{ cases of cancer expected.}$$

With this expectation, there are at least 98 chances out of 100 that 0 cases would be observed. So the probabilities are extremely strong in favor of observing 0 cases, which happened.

Conclusion.—The data are completely *consistent* with the linear thesis and completely *consistent* with the absence of any threshold "safe" dose in this range of Ra burdens, also.

The data afford *no support whatever* to either of Dr. Evans hypotheses.

The <0.001 μC Ra range

We have 42 persons in this group with a residual burden of <0.001. To favor Dr. Evans, let us use 0.001 as the median burden.

For 42 persons, therefore, our expectation is:

$$\left(\frac{42}{100}\right)\times(6.4)\times(0.001)=0.0027 \text{ cases of cancer expected.}$$

With this expectation, there are at least 997 chances out of 1000 that 0 cases would be observed. So the probabilities are enormously in favor of observing 0 cases, which happened.

Conclusion.—The data are completely *consistent* with the linear thesis and completely *consistent* with the absence of any threshold "safe" dose in this range of Ra burdens, also.

No support is obtained for either of Evans hypotheses.

Summarizing, we can state, for *all* dosages below 0.1 μC Ra, there is not a shred of scientific evidence that should lead anyone to accept either of Dr. Evans hypotheses. If evidence favoring his hypotheses exists, it certainly must be elsewhere than the data he has provided from persons with residual Ra burdens. The linear thesis and the absence of any "safe" threshold emerge totally unscathed from this analysis. They are not *proved* by this analysis, but there is no suggestion whatever that they are incorrect, *in contrast* to Dr. Evans claim. (see Quotation 4, above)

(*b*) *Analysis of the Hasterlik data* (Table 2)

The procedure of analysis of these data is identical with that provided above. For the group of cases with burdens of 1.0–10.0 μC Ra there were 14 cases of cancer out of 41 total persons.

$\frac{14}{41}$ is, therefore, the probability of cancer for a median dose of 5.5 μC Ra residual burden.

So, *per μC Ra*, $\frac{14}{\quad}$ = 0.062 is the probability of cancer.

This means 6.2 cases of cancer per 100 people are found for a residual burden of 1.0 μC Ra. This is spectacularly good agreement with the value 6.4 found for the Evans cases.

We can go through each individual group now as previously, and the results of such analysis are presented in Table 3.

TABLE 3.—ANALYSIS OF EXPECTATION VERSUS OBSERVATION IN THE HASTERLIK SERIES OF CASES (THESE ARE THE GROUPS WHERE 0 CANCERS WERE OBSERVED)

Number of cases	Dose range	Median dose (use)	Expected number of cancers	Probability of observing 0 cancers in this series
23	<0.001	0.001	0.0014	998 out of 1,000.
36	0.001–0.01	0.0055	0.012	99 out of 100.
102	0.01 –0.1	0.055	0.35	65 out of 100.

Clearly, from these analyses, we can state the data are completely *consistent* with the linear thesis and completely *consistent* with the absence of *any* "safe" threshold range of Ra burden.

These analyses provide nothing at all to indicate we should accept *either* of Dr. Evans hypotheses.

(*c*) *Analysis Based upon Use of All Cancer Cases to estimate the probability of cancer per μ C Ra*

In order to explore every possible way of analyzing the data to see if *any* support can be developed for Evans hypotheses, we thought it worthwhile to estimate the cancer probability by using all groups where cancer did occur. Using both the Hasterlik data and the Evans data, we have the combined totals shown in Table 4.

TABLE 4.—COMBINED DATA FOR ESTIMATION OF CANCER PROBABILITY ASSOCIATED WITH RESIDUAL RA BURDEN (HASTERLIK PLUS EVANS DATA)

Number of cases	Dose range	Median dose	Number of cancers observed
94	0.1– 1.0	0.55	6
81	1.0– 10.0	5.5	28
14	10.0–100.0	55.0	3

To estimate the probability of cancer per μC residual Ra burden, utilizing all cases, we need first the average burden for the overall group of persons.

$$\text{Average Burden}=\frac{(94)(0.55)+(81)(5.5)+(14)(55.0)}{94+81+14}=\frac{1267.2}{189}=6.7\ \mu\text{C}$$

Therefore, probability of cancer per μC Ra burden is:

$$\frac{6+28+3}{(189)(6.7)}=\frac{37}{(189)(6.7)}=0.029$$

But this is much lower than the 0.064 we used above. Therefore, if we used 0.029 as the probability of cancer per μC Ra, the analysis would lead to the conclusion that it is even far less likely that any support for Evans hypotheses exists within these data.

Lastly, we may exclude the people with the very high Ra residual burdens (10–100 μC Ra) on the grounds that a very high prior death rate may have left an unrepresentative group.

In this case, we exclude 14 subjects with burdens of 10 μC or more, and we calculate:

$$\text{Average Burden}=\frac{(94)(0.55)+(81)(5.5)}{94+81)}=\frac{497.2}{175}=2.8\ \mu\text{C}$$

The probability of cancer per μC Ra residual is:

$$\frac{6+28}{(175)(2.8)}=\frac{34}{(175)(2.8)}=0.069$$

But this number is so close to the 0.064 already utilized, that no material support for the Evans hypothesis will derive from its use instead of 0.064.

(d) Analysis of the Evans Series and the Hasterlik Series Combined

As Evans has correctly stated, the data from his series are in remarkably good agreement with the data of Hasterlik. In the hope that possibly, having a larger series through combining both sets of data, it might be possible to give a fairer trial to the Evans hypotheses, we have calculated the expectations using all cases from both series. As the probability of cancer per μC residual Ra burden, the mean of the values derived from Evans data and from the Hasterlik data, namely, 0.063 per μC Ra residual burden is used. The "combined" analysis is presented in Table 5.

TABLE 5.—ANALYSIS OF EXPECTATION VERSUS OBSERVATION IN THE COMBINED SERIES OF CASES (HASTERLIK AND EVANS) (THESE ARE THE GROUPS WHERE 0 CANCERS WERE OBSERVED)

Number of cases	Dose range	Median dose (use)	Expected number of cancers	Probability of observing 0 cancers in this series
65	<0.001	0.001	0.0041	996 out of 1,000.
97	0.001–0.01	0.0055	0.034	966 out of 1,000.
182	0.01 –0.1	0.055	0.63	37 out of 100.

For the dosage ranges up through 0.01 μC Ra residual burden, the answer is abundantly clear—no support whatever for either of the Evans hypotheses. Even for the higher dose range 0.01–0.1 μC residual Ra burden, the results fall *far* short of acceptable support for the Evans hypotheses. If we use the minimum statistical criterion of $p=0.05$, the analysis shows a probability 7 times too high compared with what it would take to make us accept the Evans hypotheses. On matters of such grave importance, one certainly should insist on using $p=0.01$, and in this case the probability is 37 times too high compared with what it would take to argue for acceptance of the Evans hypotheses.

Again, even using the combined series, the data are *consistent* with the linear thesis and are *consistent* with the absence of any "safe" threshold of residual Ra burden.

DISCUSSION

It is now important to return to the four quotations of Evans presented in the introduction and to show, in turn, the error in each one.

Quotation 1 (see above) claims, "these studies have provided the permissible body burden for radium in humans". The analyses presented above show that "these studies" provide nothing in the way of support for a "safe" threshold body burden with respect to cancer induction. If it is true that NCRP, ICRP, and AEC have, as Evans suggests, used these studies to decide permissible burdens of radium, plutonium, and strontium-90, they would be well advised to cease and desist from any further such use.

Quotation 2 (see above) claims it is Evans "conviction that an absolute or practical threshold exists, below which radon daughters are innocuous". A "conviction" is, of course, a strange phenomenon. It can be based upon scientific evidence, upon intuition, upon hunch, upon religious belief, or upon hope. We would be the first to defend staunchly Professor Evans' right to hold convictions based upon intuition, hunch, religious persuasion, or hope. Our analysis does not address itself to these areas. We can state that his conviction cannot rest upon scientific evidence, for our analysis shows that no such evidence exists.

Quotation 3 (see above) claims that "the RPG of 0.1 μC Ra contains a large safety factor and would appear satisfactory even if applied to large populations". This contention rests in part upon the fact that Professor Evans' studies are of *residual* radium burdens, and the suggestive evidence that the initial burden

was probably 20 times higher. Thus, he suggests that if 0.1 μC Ra residual burden is "safe", then 2.0 μC Ra *initial* burden would be safe. So, he calculates that 0.1 μC Ra *initial* burden is "conservative". But 0.1 μC Ra *initial* burden corresponds to 0.005 μC Ra residual burden. In the analyses above we have demonstrated that Evans data offer *no* support that 0.005 μC Ra residual burden is below any kind of threshold. Therefore, there is no evidence at all to support his contention that 0.1 μC Ra initial burden is *at all safe*, to say nothing of being conservative.

This being the case, his *assertion* that such a value would be satisfactory even if applied to large populations could lead, if accepted by responsible authorities, to a public health disaster unparalleled in the history of mankind.

Quotation 4 (see above) claims, "the linear, non-threshold model of radiation carcinogenesis is *conservative, oversimplified,* and *incorrect*". But our analysis shows that Evans data and his analyses do *not*—

(a) even remotely suggest the linear, non-threshold model to be conservative,

(b) even remotely suggest the linear, non-threshold model to be oversimplified,

(c) even remotely suggest the linear, non-threshold model to be incorrect.

It is *conceivable* that the linear, non-threshold model of radiation carcinogenesis may be conservative, oversimplified, and incorrect. If so, this remains for future science to demonstrate. Evans' work simply does not bear upon this issue. It *can* be stated that the linear, non-threshold model *does make* excellent sense in setting Public Health Standards for radiation exposure.

It would be irresponsibility of the highest order, repugnant to any competent bio-medical scientist, to set Public Health Standards based upon a *hope*, unfounded in evidence, that somehow a poison will turn out to be less toxic than conservative sound estimates would indicate.

REFERENCES

(1) Gofman, J. W. and Tamplin, A. R. "Low Dose Radiation, Chromosomes, and Cancer", Presented at "1969 Institute of Electrical and Electronic Engineers Symposium: Nuclear Science", Oct. 29, 1969, San Francisco, California (To be published in IEEE Proceedings, February, 1970).

(2) Gofman, J. W. and Tamplin, A. R. "Federal Radiation Council Guidelines for Radiation Exposure of The Population-at-Large—Protection or Disaster?", Testimony presented before The SubCommittee on Air and Water Pollution Committee on Public Works, United States Senate, 91st Congress, November 18, 1969.

(3) Evans, R. D. in "Radiation Exposure of Uranium Miners", Hearings of the Joint Committee on Atomic Energy, 90th Congress, Part 1, May 9 thru August 10, 1967, p. 265.

(4) Ibid p. 274.

(5) Evans, R. D. "The Effect of Skeletally Deposited Alpha Emitters in Man", *Brit. J. Radiol. 39*, 881–895, 1966.

(6) Reference 3, p. 274.

(7) Hasterlik, R. in "Radiation Standards, Including Fallout", Hearings of the Joint Committee on Atomic Energy, 87th Congress, Part 1, June 4–7, 1962, p. 325–331.

(8) Evans, R. D. "The Radium Standard for Boneseekers—Evaluation of the Data of Radium Patients and Dial Painters", *Health Physics, 13*, 257–278, 1967.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., December 22, 1969.

Hon. EDMUND MUSKIE,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: I am sending you and Senator Gravel this letter, with copies to Professor Edward Teller of LRL, Livermore and Dr. John Totter

of Division of Biology and Medicine, AEC, Washington, D.C. We are pleased that our testimony before your Subcommittee has finally awakened some scientific debate on this issue of radiation hazards. As we said to you in the testimony, we were seeking scientific comment from those who disagreed with our findings and recommendations. It would indeed be remiss of us not to let you know our *scientific* reaction to the criticisms of Drs. Teller and Totter, for both of whom we have the highest regard.

Dr. Teller's critique was submitted to Senator Gravel, with a distribution list attached.

Dr. Totter's critique was in a statement released to the press through Mr. Roger Rapoport of Dispatch News Service.

One statement of Dr. Teller's, namely that more study is needed, we agree with heartily. Essentially everything else dealing with our work, we disagree with extensively and in minute detail.

Dr. Totter's statement we disagree with in toto.

While we feel that both men's critiques were already abundantly answered in the testimony and supplementary testimony before your Subcommittee, it is clear that we have not impressed Dr. Teller or Dr. Totter with our evidence.

We are disappointed to have been ineffective in this regard, and since the matter is so vital to the welfare of the U.S. public, we must, as scientists, do a better job. Therefore, we are preparing an additional paper for your Subcommitee dealing specifically with the comments of Dr. Teller and Dr. Totter. We hope to have this in your hands shortly for review.

With Holiday Greetings,

Sincerely yours,

JOHN W. GOFMAN, M.D.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., December 29, 1969.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: My congratulations! You and your colleagues have succeeded where all others have failed. The radiation issue will finally be looked at.

As you will note from the enclosed, we are going to help Dr. Tompkins with his review even though he hasn't requested it. Perhaps as we generate supplementary material, you may wish to put it out as further supplements to your Hearings.

I feel confident that neither you nor the Subcommittee will accept perfunctory answers from the Federal Radiation Council.

With much admiration,

Sincerely yours,

JOHN W. GOFMAN, M.D.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., December 26, 1969.

(Distribution List attached)

DEAR DR. TOMPKINS: We congratulate you on your recent decision to undertake a complete and thorough review of the Federal Radiation Council Guidelines for Radiation Exposure of the Population-at-Large.

Because we are so vitally concerned about the public health and welfare in this matter, and for no other reason, we offer to assist you in every way possible

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in this review. You have stated so well publicly that we have made a direct frontal attack on all radiological biology and on all standards, including those of the Federal Radiation Council, the International Commission on Radiological Protection, and the National Committee on Radiation Protection. We *do indeed make this direct frontal attack.* For over five years we have been consistent in expressing our view that no adequate scientific foundation underlies any of these standards. Our recent researches have simply served to magnify enormously our lack of confidence in such standards.

But we have only begun, through the Hearings of the Senate Subcommittee on Air and Water Pollution, to make this attack on the standards. There is much more evidence, beyond this testimony, that we are going to provide to assist you in your review.

In the interest of facilitating the acquisition of the pertinent information by all the interested persons and agencies we shall provide copies of this letter and all supplementary documents to everyone on the attached distribution list. Should there be additional individuals or agencies that you would like to add to this list, we shall be pleased to add them.

At present, the following documents are nearing completion for your review. They will not be long in reaching you.

I. A refutation of Dr. John Storer's critique that indicated we had overestimated the expected cancer plus leukemia risk by a factor of 100. Dr. Storer suggests 160 cancer plus leukemia cases annually instead of our 16,000. We propose to demonstrate why we reject Dr. Storer's analysis and why we stand firm on our estimate of 16,000 cases.

It is of great interest that in making his own estimate of 160 cases, Dr. Storer has reduced the problem to a *numerical* difference of opinion, which we believe is readily resolved. The mythology of "enormous built-in conservatism of FRC Guidelines" appears no longer to be defended.

II. A refutation of two statements publicly quoted as representing Dr. John Totter's position on our findings. (AEC–DBM)

(a) That we have overestimated the cancer-leukemia risk by a factor of 100. The refutation (I) of Dr. John Storer's analysis will suffice to answer this.

(b) That we have largely ignored crucial animal data which will mitigate the risk. We reject the suggestion that *any* experimental animal data provide information relevant to an alteration of our estimates.

III. A refutation of Dr. Robley Evans latest evidence claiming the linear-non-threshold model of radiation carcinogenesis in man is incorrect. We have already refuted Dr. Evans analysis resting upon residual radium burden. More recently Dr. Evans suggests a more appropriate basis for consideration to be the *cumulative rads* delivered. We like and accept Dr. Evans suggestion that cumulative rads be used and we shall present you in detail an analysis on that basis. We shall demonstrate:

(a) Based upon cumulative rads, no evidence whatever exists for a safe practical *or* absolute threshold of radium exposure of man.

(b) No challenge whatever exists to the linear model of radiation carcinogenesis even for total doses 100 times as high as those pertinent for the FRC Guidelines.

IV. A complete analysis of the Uranium Miner Story (prepared at the suggestion of Dr. Carl Walske of the FRC). This analysis will demonstrate:

(a) The sound advice of such eminent scientists as Dr. Walter Snyder and Mr. Herbert Parker at the extremely valuable JCAE Hearings apparently went largely unheeded in setting new standards for uranium and hardrock mining exposure to radon and radon daughters.

(b) The new FRC Guidelines for uranium and hardrock mining need total review and revision.

V. An analysis of the Marshall Island cancer situation. As you know, until recently much comfort was taken in the absence of radiation-induced thyroid

cancer there. This has now been totally changed by the recent announcement of Dr. Robert Conard of the Brookhaven National Laboratory that invasive cancer of the thyroid is now definitely occurring in the Marshallese. Consideration of latency in the induction of cancer suggests of course, that this time sequence is precisely what should have been expected.

We shall demonstrate that the Marshall Island data provide another important buttress to our generalizations concerning the dose-effect relationship for radiation carcinogenesis in man.

VI. An analysis of the radiation induction of breast cancer in humans. Dr. John Storer, quoting Dr. Robert Miller, has suggested that the ABCC data on breast cancer induction may not be firm. We have already refuted this in our testimony. However, we shall demonstrate by further analysis, utilizing the important evidence of MacKenzie, that

(a) Breast cancer induction by radiation in humans is, indeed, firm.

(b) The data concerning quantitative aspects are consistent with our generalizations for radiation carcinogenesis in humans.

VII. An analysis of the radiation induction of lung cancer. Dr. Storer, quoting Dr. Robert Miller, has suggested that the ABCC data concerning radiation induction of lung cancer in Japan is subject to doubt, based upon the histology of the cancers. We have already refuted this in our testimony. However, valuable supplementary refutation will be available through an analysis of the Uranium Miner Story (IV). We shall demonstrate:

(a) There is no reason to doubt the Japanese data.

(b) Radiation not only clearly induces small cell cancer of the lung, but also induces bronchiogenic cancer, and this is demonstrable from Court-Brown and Doll data, the ABC data, *and* the Uranium Miner data.

Should any additional evidence come to your attention that we are not aware of, we shall appreciate the opportunity to assist your review by any additional analyses we might provide.

We would hope that an early resolution of the scientific issues concerning *risks* of radiation for population-at-large can be made now. It is important to segregate the risk issue from the possible benefit issue for any program involving atomic energy. Speaking strictly for ourselves, and in no way representing our Laboratory, we must say we believe that the intertwining of the benefit and risk issues by the Federal Radiation Council has been a grave error. The scientific community must help the Federal Radiation Council in every way possible to reach the best estimates of risk. The problems of potential benefits we may, as citizens, receive, in exchange for subjecting ourselves to some established risks, is far more appropriately handled in the Congress of the United States. There the public, through its elected Representatives and Senators, can make its views known concerning what price they wish to pay for specified societal benefits.

You may be assured of our deepest commitment to assist you in every possible way in fulfillment of your responsibilities in this crucial area.

With kindest regards.

Sincerely yours,

JOHN W. GOFMAN.
ARTHUR R. TAMPLIN.

DECEMBER 26, 1969.

Memorandum to: Dr. Paul Tompkins, Executive Director, Federal Radiation Council.

From: John W. Gofman and Arthur R. Tamplin.

Subject: "The Federal Radiation Council Review of Radiation Standards for Population Exposure."

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APPENDIX II

Agency Comments on Testimony of Gofman and Tamplin (page 58)

Contents:

(The materials which follow represent letters, views, and comments by the International Commission Radiological Protection, National Council on Radiation Protection and Measurements, AEC, the Department of Agriculture, and the Department of Commerce on the statements and testimony presented by Drs. Gofman and Tamplin at the hearing:)

International Commission on Radiological Protection

INTERNATIONAL COMMISSION ON RADIOLOGICAL PROTECTION,
January 15, 1970.

Senator EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: I am replying to your letter of 1st December, which only arrived here yesterday. In veiw of your request for an early reply, I am anwering your letter personally, though you will realise that I have not had an opportunity to consult the Commission on this matter.

The basic sources of information used by Drs. Gofman and Tamplin are of course familiar to the Commission, and, together with other sources, many of them were used during the preparation of the Commission's report "The Evaluation of Risks from Radiation" (ICRP Publication 8), which in turn influenced the drafting of the Commission's current Recommendations (ICRP Publication 9), with which I am sure you are familiar.

In ICRP Publication 9, the Commission makes certain recommendations for dose limits, both for individuals and for populations, and it discusses the fundamental principles upon which these are based. While it is essential to consider the entire discussion in its context, it might be useful to emphasize a few sections of the report that seem to be especially relevant to the present issue.

On the question of threshold and linearity of effect with dose, the Commission has this to say:

"(7) The mechanism of the induction by radiation of leukaemia and other types of malignancy is not known. Such induction has so far been clearly established after doses of more than 100 rads, but it is unknown whether a threshold does exist below which no malignancy is produced. If such a threshold dose did exist, there would be a no risk of the induction of malignancy, as long as the threshold was not exceeded. As the existence of a threshold dose is unknown, it has been assumed that even the smallest doses involve a proportionately small risk of induction of malignancies. Also, because of the lack of knowledge of the nature of the dose-effect relationship in the induction of malignancies in man—particularly at those dose levels which are relevant in radiological protection—the Commission sees no practical alternative, for the purposes of radiological protection, to assuming a linear relationship between dose and effect, and that doses act cumulatively. The Commission is aware that the assumptions of no threshold and of complete additivity of all doses may be incorrect, but is satisfied that they are unlikely to lead to the underestimation of risks. Information is not available at the present time which would lead to any alternative hypothesis."

The Commission discusses the acceptability of risk, as follows:

"(34) Any exposure to radiation is assumed to entail a risk of deleterious effects. However, unless man wishes to dispense with activities involving exposures to ionizing radiations, he must recognize that there is a degree of risk and must limit the radiation dose to a level at which the assumed risk is deemed to be acceptable to the individual and to society in view of the benefits derived from such activities. Such a dose might be called an acceptable dose, with the same meaning as was implied by 'permissible dose'.

"(35) If the quantitative relationship between dose and the risk of an effect were known, societies or individuals could judge the degree of risk that would be acceptable, taking into account the particular circumstances requiring a radiation exposure. Ideally, such a judgment would involve a balancing of the benefits or necessities of the practice against the risks of the given exposure, which could also be related to that of other risks in the particular society. In addition, the difficulties of limiting the exposures would have to be taken into account.

"(36) If the dose-effect relationship were known, and if it were possible to decide on a degree of risk that would be considered acceptable in a particular circumstance, it would thus be a secondary matter to fix the acceptable dose that would correspond to this risk. However, at the present time the relationship between dose and risk is not known with precision, nor is it usually possible to make quantitative evaluations of the benefits. Despite this, and in view of the continuing need for practical advice for planning purposes, the Commission recognizes its responsibility to maintain its practice of recommending appropriate dose limitations."

Concerning the dose limitation for radiation workers,

"(38) Long experience in the use of X-rays, radium and other radioactive materials, together with information on radiation injuries in man and other organisms, has indicated that values for Maximum Permissible Doses can be set such that there is a low probability of radiation injury without undue restriction of the uses and benefits of ionizing radiations. These facts form the present basis of the Commission's recommended values."

For members of the public,

"(42) It is not desirable to expose members of the public to doses as high as those considered to be acceptable for radiation workers; members of the public include children who might be subject to an increased risk and who might be exposed during the whole of their lifetime; members of the public (in contrast to radiation workers) do not make the choice to be exposed, and they may receive no direct benefit from the exposure; they are not subject to the selection, supervision and monitoring required for radiation work, and they are exposed to the risks of their own occupations.

"(43) The amount by which dose limits for members of the public should be less than those set for radiation workers depends upon factors for which no commonly accepted quantitative values exist. However, for planning purposes it is considered appropriate to set the dose limits for members of the public a factor of ten below those for radiation workers. No undue biological significance should be attached to the magnitude of this factor, as at present the radiobiological information in this respect is inadequate."

For populations, concerning somatic risks,

"(95) The Commission's previous recommendations gave no value for a maximum 'somatically significant' dose for a population, as no such dose could easily be defined or estimated. However, it is possible to define a population dose for a particular risk, such as leukaemia, on the assumption that the dose-effect relationship is linear without a threshold; on this basis the relevant dose is presumably mean dose to active red marrow, appropriately averaged over each individual in the population. On these assumptions it is possible to estimate, for example, the number of cases of leukaemia in a population receiving an annual mean dose of 0.5 rem to the red marrow (corresponding to the annual dose limit for members of the public). Risk estimates indicate that at equilibrium there might be an increased incidence of leukaemia amounting, at the most, to about ten cases per year per million persons exposed. The Commission believes that improved knowledge of risk estimates will eventually assist national authorities to assess the acceptability of a somatic dose to a population. In the meantime, however, it is expected that the dose limits for individuals will ensure that the number of somatic injuries that could possibly occur in a population will remain at a low level."

It needs to be reiterated that the assumption of linearity of effect with dose has still to be proved at the dose levels covered by the Commission's dose limits. This matter is discussed fully on page 101 of the Commission's task group report "Radiosensitivity and Spatial Distribution of Dose" (ICRP Publication 14). The task group concluded:—

". . . it is unfortunate that there is so little evidence by which the adequacy of linearity can be tested over the dose range from 100 to 1000 rads."

The Commission keeps under continual review the radiobiological bases for radiation protection, and it amends its recommendations in the light of new knowledge. At the present time there is no proposal within the Commission for a general lowering of its recommended dose limits.

I hope that you will let me know if I can be of any further assistance.

Yours sincerely,

F. D. SOWBY.

ICRP PUBLICATIONS 8, 9 AND 14

Publication 8: The Evaluation of Risks from Radiation. A Report prepared for ICRP Committee 1. Pergamon Press (1966). Also *Health Physics*, Vol. 12, No. 2 (1966).

Publication 9: Recommendations of the International Commission on Radiological Protection (Adopted September 17, 1965). Pergamon Press (1966).

Publication 14. Radiosensitivity and Spatial Distribution of Dose. Reports prepared by two Task Groups of ICRP Committee 1. Pergamon Press (1969).

National Council on Radiation Protection and Measurements

NATIONAL COUNCIL ON RADIATION PROTECTION AND MEASUREMENTS,
Washington, D.C., December 15, 1969.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air Pollution,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: Responding to your letter of December 1, we were aware of the hearings being held by the Subcommittee on Air and Water Pollution. In fact, I received an invitation from Senator Gravel inviting me to comment on a number of questions that would be posed. Unfortunately, it was impractical to do this in advance of the hearings, and I explained to him that this came at an especially awkward time in light of the many immediate prior commitments that we have.

I face the same difficulty in regard to your question, namely, that to make adequate response to the testimony of Gofman and Tamplin would require much more time than any of us have available at the moment. In the meantime, I am presenting your letter to our Board of Directors which meets this week and I would be happy to apprise you of any position that they may arrive at, or any decision as to how much time we can afford to spend on responding to such strange testimony. The difficulty is that Messrs. Gofman and Tamplin are not recognized as having any special competence in this field. They are presumably good scientists and they are entitled to their opinions, but the scientific and judgmental content of those opinions are at best questionable. You can expect to hear further from me.

Sincerely yours,

NATIONAL COUNCIL ON RADIATION PROTECTION AND MEASUREMENTS,
Washington, D.C., January 23, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: This is further to my letter of December 1, responding to your request for comments on some of the testimony offered by Drs. Gofman and Tamplin. However, in replying to your inquiry it is important that you be provided with a summary of what is involved in the establishment and promulgation of radiation protection standards.

The setting of numerical radiation protection standards is by no means completely amenable to a direct scientific approach. While probably more is known about radiation than about any other environmental pollutant, there are still deficiencies in our biomedical knowledge. For example, the radiation effects in the low dose and low dose-rate region are so small and infrequent that they have thus far escaped detection by direct observation, statistical analysis or epidemiological studies. To establish some *range* of the magnitude of possible effects, if any, it is necessary to make certain assumptions regarding dose-effect relationships in the region where direct observations are lacking, e.g., in the range of occupational or population exposure levels that have been used over the past 35 years.

The most conservative assumptions that might be applied in the low dose and low dose-rate region would be (1) there is a single linear relationship between dose and effect for all effects, (2) the effects are independent of dose rate from zero dose up to levels where the effects are observable, and (3) there is no threshold level of dose below which no radiation effect occurs. By implication, these assumptions make all doses of all magnitudes to a given site cumulative, and deny the possibility of any recovery process. These are contrary to the facts; it is well known that this conservative assumption does not hold generally. However, in rigid interpretation, it has been used by some to calculate a theoretical *upper* limit of incidence of all effects in man. It is generally believed that the actual effects of low doses and low dose-rates lie someplace between zero effect and that obtained at high dose and dose rates. The question is not *whether* linear interpolations from high dose and dose rates overestimate the degree of effect at low doses, *but only by how much.*

Evidence continues to mount that the upper limits based on the above assumptions are probably excessive by substantial factors, the magnitudes varying with the particular effect. After the available information is evaluated as far as the scientific evidence and judgment permits, there still remain questions involving such things as risk comparisons, economics, philosophy, social impact and politics—none of which are amenable to direct and unequivocal scientific or mathematical evaluation.

It might be helpful for you to have some information on the historical development of radiation protection standards.

The first such standard in this country was developed in the early 1930's by what is now called the National Council on Radiation Protection and Measurements (NCRP); the standard of 0.1 roentgen per day for radiation workers was less than a thousandth of the dose required to produce an observable skin erythema. The first important modifications of this were made by the NCRP in the late 1940's, and the recommendations of that time were later adopted internationally. The new standard (0.3 roentgens per week) was based primarily on the projected wider use of radiation and concomitant larger number of persons exposed.

The most recent changes of significance were also introduced by the NCRP, in 1957. A revision downward at that time to an average of 5 rem per year was based primarily on the rate of increase in the genetic effects of radiation then observed at doses and dose rates much higher than normally encountered by persons engaged in radiation activities. At the same time a level of 0.5 rems per year was set for individuals in the general population. More recent experimental data, obtained with a lower range of doses and dose rates, indicates that in 1957 we had probably overestimated the frequency of the genetic effects. Nevertheless prudence, and the fact that we can operate within present limits, dictates that we not consider raising the population dose limits until considerably more data are available. The 1957 modifications were adopted by the International Commission on Radiological Protection (ICRP) about a year later and in 1960 by the Federal Radiation Council (FRC). Since that time there have been no major changes in the basic radiation protection standards in this country or in the

The development of radiation protection standards has involved extensive scientific expertise and high level professional judgment. This in turn has necessitated that the involved individuals have long, continuing experience in the field and the benefit of debate and discussion with colleagues and peers from all over the world. As a result, the recommendations that have been developed have been subjected to extensive and critical scrutiny. At the same time the various protection bodies developing the standards have been made up of carefully selected people representing virtually all technical and professional points of view to be certain no "blind spots" are overlooked.

Following the changes introduced in 1957, the NCRP reestablished its Scientific Committee on Basic Radiation Protection Criteria. For the past twelve years this committee has had under continuing review all of the new radiation effects information that has been developed throughout the world and which might be relevant to radiation protection standards. This effort has included close contacts with international organizations, such as the ICRP, United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR), World Health Organization (WHO) and the International Atomic Energy Agency (IAEA). We do not believe that there have been any important data relative to the effects of radiation which have not been known to our Council members. I might remark at this point, that included in our Council or its Committees are, or have been, most of our nationally recognized radiation protection experts, among whom there are many who are also known internationally for their expertise.

I should point out that since 1957, we have found no information or data that leads us to deviate seriously from the general principles expounded at that time. We recognize that our current permissible dose standards are based on technical factors that probably are *over conservative*, but at the same time they are considered acceptable in comparison with standards for controlling other environmental risks. In arriving at an acceptable value for radiation exposure of the population we have used our joint best judgment as to actual risk, taking into account the technical, social and economic factors which influence this judgment.

The report of our Scientific Committee on Basic Radiation Protection Criteria, now on its tenth draft, is being reviewed by the members of the Council. The majority of the members have already approved the report and at the present time it is receiving final editorial scrutiny. The report will probably not be publicly available for some months yet; when it is approved by the Council we will be glad to send you a copy.

In 1959, and in parallel with its study on basic protection criteria, the NCRP established an ad hoc committee on Somatic Radiation Dose for the General Population. Among other things it reviewed the NCRP position with regard to the assumptions used when technical and scientific knowledge was lacking. A copy of the report is enclosed.

Because it has become evident that the basis for the current permissible levels are misunderstood by some, the Board of Directors of the National Council on Radiation Protection and Measurements decided at its regularly scheduled meeting held on December 18, to reactivate the 1959 committee on Somatic Radiation Dose for the General Population. The prime purpose is to discuss and explain the problem in a manner which can be understood by the lay public and by scientists who are not familiar with radiation protection history and philosophy. We are hopeful that this objective can be accomplished to the point of eliminating any possible misuse or misunderstanding.

With regard to the testimony by Gofman and Tamplin, only general comment on principles rather than details is warranted. Their material has presented no new data, new ideas or new information and quotes only some of the facts now known in this area, while other essential data are omitted. The same basic information that they have available to them has been studied continuously by large numbers of highly experienced radiation protection scientists over at least the last two decades. The various protection bodies that have used this information have been careful to specify the limits within which it may be regarded as valid or invalid.

Drs. Gofman and Tamplin present some arithmetical exercises based on some of the planning assumptions used by all protection organizations. However, they carefully avoid the fact that the proper utilization of these assumptions results not in a single, unambiguous dose limit but in large ranges of dose limits. They conclude, as a result of their analysis that the standards should be lowered by a factor of at least 10. This conclusion is obtained on the absis of their adopting [illegible]

Furthermore, they do not make the necessary distinction between, and allowance for, protracted exposures at low dose-rates and isolated exposures at very brief high dose-rates. Therefore many of the data to which they refer (the use of which is also doubtful on other grounds) are not relevant to their argument. They also base much of their thesis on the patently erroneous assumption that all human cancers can be radiation induced in the low dose range, and that the "doubling dose" is approximately the same for all cancers.

Since you are in the very difficult position of a legislator trying to evaluate the rather complicated technical recommendations of a scientist, I can give you a lead as to the value of the Gofman and Tamplin testimony. In the material presented by them on November 18, there was the clear implication that the current radiation protection standards involved the fundamental issue of, and were based upon, the existence of a threshold of dose below which no injury could result. In their statement, "Studies of Radium-Exposed Humans", they state directly and specifically that the ICRP, the NCRP and the FRC standards or guidelines rest upon the belief by those bodies that such a threshold does exist.

The inclusion of any such erroneous statements in a technical discussion serves only to undermine the credibility of the entire work. I can assure you that you cannot find a single statement by any of those three organizations to the effect that their recommendations on protection standards are based on a threshold phenomenon. On the contrary, all of these bodies go to considerable pains in discussing the threshold phenomenon and to explain why it cannot be used and what we do about the dilemma in which this leaves us. (I can vouch for this since I have served with the ICRP for 41 years, with the NCRP for 40 years, and with the FRC for about 5 years.)

It is worthy of note that Drs. Gofman and Tamplin have not utilized the usual channels for setting forth their ideas for professional scrutiny—namely, publication in relevant technical journals that submit material to critical referee review before acceptance for publication. Nor have they submitted any proposals for consideration by the NCRP, ICRP or (until the last few weeks) the FRC. I have explained above, the arduous process that is involved in the development of radiation protection standards; we know of no effort on their part to submit their ideas to this essential process. We are aware of their engagement in work related to radiation hazard and some of their analytical work has undoubtedly been utilized one way or another by protection groups. But the same is the case for many other laboratories and laboratory workers around the world.

The possibility of deleterious effects of radiation on our environment is important, but we have reached the point in our social development where we must become dependent upon certain radiation uses even at the risk of some ill effects. It is therefore extremely important that the subject be treated with great care by informed scientists—by informed politicians—by informed legislators—by informed economists—by informed sociologists, and by many others. In this respect your committee has a very important role to play, especially in helping to provide those factors which are not strictly scientific in nature. It is for that reason that I urge that you be extremely cautious about how you treat information on radiation effects—especially those caused by low doses—which is presented to your committee, whether by Drs. Gofman and Tamplin, by us—or by anybody else.

This letter has been reviewed and approved by the full Board of Directors of the NCRP.

Sincerely yours,

LAURISTON S. TAYLOR.

Enclosure.

[Reprinted from Science, Feb. 19, 1960, vol. 131, No. 3399, pp. 482–486]

SOMATIC RADIATION DOSE FOR THE GENERAL POPULATION

The Report of the Ad Hoc Committee[1] of the National Committee on Radiation Protection and Measurements, May 6, 1959

At its meeting in November 1958, the executive committee of the National Committee on Radiation Protection and Measurements undertook to re-examine the problem of exposure of the population to man-made radiations from the point of view of somatic effects as distinct from genetic effects. This review was under-

[1] The members of the Ad Hoc Committee of the National Committee on Radiation Protection and Measurements are: Austin Brues, Argonne National Laboratory; James Crow, University of Wisconsin; E. B. Lewis, California Institute of Technology; Karl Z. Morgan, Oak Ridge National Laboratory; W. S. Snyder, Oak Ridge National Laboratory, *alternate*; Clinton Powell, U.S. Public Health Service; Frederick Seitz, North Atlantic Treaty Organization; Forrest Western, U.S. Atomic Energy Commission; and Hymer L. Friedell, Western Re[illegible] University, *chairman*. Lauriston S. Taylor, chairman of the National [illegible]

taken because of the widespread public concern over the possible effect of radiation from fallout on the population, and because of the possibility that there might be some new, definitive information regarding the somatic effects of chronic low-level radiation on man.

The NCRP was unaware of any new basic information on somatic effects of radiation, upon which it could with sound reason recommend specific changes in permissible exposures for individuals or for population groups. The NCRP felt that information relative to the question was essentially the same as that outlined in National Bureau of Standards Handbook 59. However, it appeared desirable to make a new and independent examination of the problem for the purpose of affirming the views of the NCRP. For this purpose, the NCRP established an Ad Hoc Committee to examine the question further.

At its inception, the National Committee on Radiation Protection and Measurements centered its activities primarily around the problem of radiation hazards associated with industrial and medical uses of radiation. During succeeding years, it became increasingly apparent that NCRP could not ignore its responsibility for making recommendations concerning radiation exposure of larger population groups. Cognizance was taken of this problem at various times—for example, in NBS Handbook 59 (issued 24 September 1954), on pages 78 and 79, in the paragraphs "Non-occupational Exposure of Minors" and "Number of Exposed Individuals" as well as in the separate paragraph found at the end of the Handbook. On 8 January 1957, the preliminary statement released by NCRP setting forth its revised philosophy on the maximum permissible radiation exposure to man suggested a certain limit for average gonadal exposure of the population. The addendum to NBS Handbook 59, dated 15 April 1958, contained additional recommendations concerning the maximum permissible dose to individuals outside controlled areas, and attributable to normal operations within controlled areas, for both external radiation exposure and internally deposited radioactive materials. In its statement of 28 April 1959, the use of the same maximum permissible dose (MPD) was extended to individuals in the population-at-large.

The Ad Hoc Committee report, the NCRP believes, serves to reaffirm the broad policies of the NCRP with regard to basic permissible dose criteria, but the report is not to be regarded as containing specific recommendations by the NCRP.

The report takes the line of conservatism. The Ad Hoc Committee felt that there was no other choice until more and better information is available on the effects of low-level chronic radiation exposure. Although a conservative and possibly pessimistic assumption with regard to radiation effects has been made, this should not carry any implication that either the NCRP or the Ad Hoc Committee accepts such assumptions as established facts. These assumptions have been adopted in the interests of prudence.

Upon review of the Ad Hoc Committee's report, it was noted that while the report suggests a basis for expressing the maximum permissible somatic dose for the population, it does not contain specific recommendations immediately applicable as maximum permissible doses. It also appears likely that the maximum permissible doses that might be derived from the Ad Hoc Committee's report would not be widely different from the current recommendations of the International Commission on Radiological Protection even though they are expressed in reference to another base. This report is, therefore, being referred to NCRP subcommittee I on "Maximum Permissible Dose Criteria" for further consideration and the possible formulation of specific values to be recommended as the maximum permissible dose. Pending the possible formulation and approval of such recommendations, the NCRP recommends the use of the current recommendations of the ICRP concerning permissible doses for the population.

REPORT OF AD HOC COMMITTEE

I. Introduction

The National Committee on Radiation Protection, in the past, has recommended maximum permissible doses of ionizing radiation for occupationally exposed persons and other special groups. Its recommendations regarding exposure of the whole population to radiation have been primarily concerned with the genetically significant dose. An increasing number of sources of man-made radiation, industrial and military, make it desirable to consider the setting of maximum

permissible levels of somatic exposure for the general population. This becomes increasingly important in view of the fact that certain radioelements, such as strontium and iodine, are nonuniformly distributed in the body and result in much larger doses to specific body tissues than to the gonads. This Ad Hoc Committee was appointed to examine the problem and report to the National Committee on Radiation Protection.

The Ad Hoc Committee has considered the basic concepts and premises by which maximum permissible levels of ionizing radiation for the general population might be established and how these might be applied to radiostrontium and other widespread contaminants.

II. Dose-Effect Relationship at Low Doses

Radiation doses to which the general population is likely to be exposed in peacetime are very low. Furthermore, the rate of delivery from most sources is slow, so that a small dose is accumulated over a long period. Yet the existing data upon which present protection criteria are based are from experimental animals exposed at higher, and frequently from acute, doses. Similarly, human data that are available are also primarily from higher doses.

If we understood the exact mechanism of the interaction of radiation and biological tissue, and the subsequent chemical, physiological, and morphological events leading to the final effects, we could extrapolate back to very low doses and make confident estimates of the extent of human damage to be expected from such a dose. Lacking this information, we must rely on the character of the dose-effect curve at higher doses and estimate the effects of changes in intensity and spacing of the dose.

A proportional (linear nonthreshold) relation between dose and biological effect is usually taken to imply a single-event process, especially if this is supported by data showing dose rate independence. More accurately, the relationship is $\log S = -kD$ (where S is the proportion not effected, D is the dose, and k is a constant). At low doses, this is not distinguishable from a straight line. With such a dose-effect relationship, linear interpolation between the observed values and the origin is acceptable when the doses and the related effects are too low to be measured accurately with our present methods.

If the true relationship is curvilinear at low doses, or if there is a threshold dose below which no effect is produced, a more complex mechanism may be inferred and extrapolation to lower doses could be grossly misleading.

The committee concludes that the present data are still *insufficient to establish the character of the dose-response* curve for somatic effects. Nor is there sufficient knowledge of the mechanisms to serve as a guide in areas where the data are not available.

In the absence of such information, the committee believes that it is prudent to be conservative and choose a premise which, if in error, would be likely to overestimate the effect of low doses rather than underestimate it. The committee decided to adopt as an assumption that a proportional relationship between dose and effect exists, as briefly outlined above. This signifies that no threshold exists, and, by inference from some of the theoretical concepts, we will assume further that the radiation dose is completely cumulative and that the effect is independent of the rate at which the radiation is delivered.

If there is a threshold, there will be no effect at doses below this threshold value. If the true relation is curvilinear with an accelerating effect as the dose increases, such as would occur if the biological effect depended on multiple events or on a mixture of threshold and nonthreshold causes, the proportional assumption overestimates the effect at low doses. There is the possibility that the curve is concave in the opposite direction, but this seems very remote. Moreover, data that show a dose-rate dependence generally indicate that the effect is less with a low rate of delivery or with intermittent dosage than with the same total delivered in a short time. For these reasons, the committee believes that the proportional assumption is a conservative, and perhaps a stringent one.

The Ad Hoc Committee emphasizes that this conservative assumption was adopted not because any definitive conclusions were reached as to the true nature of the dose-effect relationship but because the committee would prefer to err on the side of overcaution rather than in the opposite direction. With this assumption (nonthreshold linear dose-effect relationship), or, for that matter, any nonthreshold assumption, it follows that even the smallest dose would involve some risk. This means that the exposure should be kept as low as feasible

and that no level of radiation is warranted unless the benefits balance or outweigh the assumed risk.

This also means that if a maximum permissible dose is determined, it will necessarily be at an arbitrary level where, in the judgment of those choosing the level, the risk is acceptable as compared to the benefits. Every effort should be made to maintain the acutal dose as far below the permissible level as possible.

III. Should the Population Dose Differ From That for Occupationally Exposed Groups?

The committee believes that the dosage permitted for the general population shold be substantially less than that permitted for occupationally exposed or other special groups. Some of the reasons are:

(1) The general population is much larger, and if exposed to the same dosage there will be the risk of a correspondingly larger number of individuals with injurious effects.

(2) Employment involving occupational hazard to exposure is voluntary, and the extent and nature of the exposure can, in principle, be foreseen by the individual accepting any risk that may be involved.

(3) Industrial workers are relatively carefully screened. Generally, those least able to meet any peculiar hazard may be channeled into other activities.

(4) In industry there can be specific evaluation and control of the hazards by radiation monitoring and other studies.

(5) Children and embryos may be particularly sensitive. These can generally be excluded from groups receiving the maximum permissible occupational dose.

(6) The number of years of exposure to radiation for occupational reasons will be much less than the number of years of exposure to environmental sources of radiation.

(7) If industrial hazards exist, it is obvious that any of these hazards (one of which is radiation) should not be spread beyond the individuals in that particular occupation. If the hazards to the outside nonindustrial population are not reduced as compared to those within the industry, the risk to the total population could be unacceptably high because of the contributions from all the occupational hazards in the society.

For these reasons, the committee believes that it is appropriate to set lower maximum permissible doses for general population groups than for persons exposed to radiation for occupational reasons.

IV. Bases for Establishing a Maximum Permissible Dose

On the basis of the assumption discussed in section II, any realistic recommendations of maximum permissible dose must be reached by balancing biological risks against the reasons for accepting exposures to radiation. It is highly improbable that such a balance can be made with accuracy, not only because of our limited knowledge, both of benefits and of risks, but also because of difficulties in comparing social, economic, and other benefits with radiation risks. Nevertheless, since decisions will be made, if only by default, it is desirable to make the best evaluations possible at the present time.

As a first approach, there are several possible scales on which the risks from low levels of radiation dose may be related to human experience. The committee believes that all of these are meaningful and has tried to consider them in its deliberations.

(1) *Relating the population dose to the level established for occupationally exposed groups.* This could be done by taking an arbitrary fraction of the occupational dose and using this as the maximum permissible dose for the general population.

(2) *Relating population dose to the estimated effects of radiation and to other risks of life.* The estimated effects of the exposure of the public to low doses of radiation can be assessed in principle in three ways: (i) by their estimated absolute incidence; (ii) by their estimated incidence relative to the spontaneous incidence of the same biological effects, *e.g.* leukemia; and (iii) by comparison with the effects of other population risks not associated with radiation.

(3) *Relating the population dose to the natural background radiation level.* If any risks are associated with natural backgorund radiation, they are accepted as a normal factor of life. Ordinarily no effort is made to reduce them, and ordinarily no consideration is given to differences in background levels in determining where one shall reside.

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The committee recommends, pending more precise information, that maximum permissible doses for the general population should be related to the average natural background level of radiation. One reason is that this level can be determined relatively easily and is relatively stable in time. A more important reason is that this is a level to which the human population has been exposed throughout its history. The further we get from this level, the less confidence we have that any effects will be similar in kind and quantity to those the population has experienced from natural background radiation and has been able to tolerate in the past.

V. Recommendations Regarding Permissible Doses to the Population

It is not the responsibility of this Ad Hoc Committee to recommend specific levels of maximum permissible dose to the population. It hopes that as more data become available, both as to benefits and risks, a maximum permissible dose representing a proper balance between these can be found. Meanwhile, it believes that the maximum permissible dose of man-made radiation (excluding medical and dental sources) should not be substantially higher than the background level of natural radiation without a careful examination of the reasons for higher values. For this purpose it may be convenient to take the background level arbitrarily to be 100 millirem per year.

In the practical application of maximum permissible levels to the general population, it is necessary to consider a number of factors, some of which are noted in the following discussion.

It is not feasible at the present time to monitor the population dose solely by measuring the dose to individuals. Moreover, any control measures to be effective must be directed at levels of radiation and of radioactive materials in the environment. Thus, it is contemplated that maximum permissible levels for such environmental factors as food, water, and air will be set for certain areas in such a manner that the radiation dose to typical persons in those areas from all sources (excluding natural background, medical, and dental sources) will not exceed the appropriate maximum permissible level. For this purpose the committee recommends that it should be allowable to average doses over a suitably long period of time, *e.g.* one year, and over population groups approximating the size of a state or major city. Because of variability of dose levels with location, it is expected that the average dose to the total population would be considerably less than the maximum permissible level.

Some radioisotopes are distributed through the body in such a fashion as to give an approximately uniform distribution of radiation dose to all of the body tissues. However, from certain radioisotopes, such as those of strontium and iodine, radiation doses are much higher in some tissues than in others. In general, the maximum permissible level should apply to the tissue receiving the greatest dose—bone in the case of strontium, thyroid for iodine. If several sources of radiation are involved, the total dose to the tissue from all such sources should not exceed the maximum permissible level.

It is recognized that for some radioisotopes, environmental levels may conceivably result in higher radiation doses to children than to adults. In such cases, permissible levels should apply to radiation doses received in the age ranges of highest dose, rather than to the population group as a whole.

The committee emphasizes that the final criterion in environmental control is the level of radiation dose to human tissues, and that environmental levels are used as indicators and means of control. At the present time permissible levels for the environment may be derived from permissible levels of dose to humans only by making certain assumptions involving such factors as movement of radioisotopes in the environment, relationships, between environmental and dietary concentrations, and biochemical behavior in the body. Recommended maximum permissible concentrations in the environment will require revision as new information on such factors becomes available, or as indicated by actual experience with environmental situations.

Since any maximum permissible level based on the considerations discussed above is a relative standard designed to keep the average radiation dose to the population as low as feasible, it follows that *a level recommended for one set of conditions may not be appropriate for another.* For example, maximum permissible concentrations in foods designed to limit the release of radioactive materials into the environment may appropriately be much lower than levels at which the foods may be considered unfit for use; and maximum permissible

concentrations in air designed to limit the release of materials into the environment may be much lower than levels at which it would be wise to evacuate an area in case of accidental release of larger quantities of such materials.

VI. Discussion

This committee has not made any recommendations regarding medical and dental radiation. The reason is that in this case the individual exposed to the risk and the one receiving the benefit are the same. The balancing of the risk is largely a medical problem. Furthermore, there are circumstances when going beyond any preassigned maximum permissible level may be thoroughly justified. It is axiomatic that every reasonable precaution should be exercised to keep the radiation dose as low as possible.

The Ad Hoc Committee was not asked for comments regarding genetically significant radiation. With the assumption of an effect proportional to the dose, which is the same as is generally assumed for genetic effects with low doses, some of the genetic and somatic considerations become very similar. Some sources of radiation, such as radioactive cesium, give about the same dose to the gonads as to other parts of the body. For others, such as radiostrontium, the gonad dose is exceedingly small in comparison with the bone dose.

The committee would like to note that if the National Committee on Radiation Protection chooses a maximum permissible dose of man-made radiation, exclusive of medical and dental sources, in the general vicinity of the background level, there will be an order of agreement with the recommendations of other groups that have studied the problem. The previous recommendation of the National Academy of Sciences Committee and the National Committee on Radiation Protection for a maximum *average-per-capita-dose* to the gonads of 10 roentgens of man-made radiation per 30 years is roughly three times the background level, and these recommendations include the estimated contribution from medical and dental radiation. We note the maximum permissible dose of whole-body exposure for a single individual recommended for the general population by the International Commission on Radiological Protection and the NCRP, although expressed in terms of a fraction of the permissible occupational exposure, is approximately five times the background. For long-range planning purposes, the International Commission on Radiological Protection has suggested a permissible average level for the whole population in the general vicinity of the background dose (a man-made radiation level of 1.7 times background, if background is taken to be 100 millirem per year).

VII. Summary: Conclusions

On the basis of the general principles outlined previously, and examination of some of the problems posed by widespread man-made contamination by various radioelements, the committee makes the following recommendations for the guidance of those concerned with the establishment of tolerable somatic levels for widespread radiation:

(1) The committee believes that present evidence *is not sufficient* to establish the dose-response curve for somatic effects at low doses. In the absence of such information, the committee has chosen to make the cautious ***assumption*** that there is a proportional relation between dose and effect and that the effect is independent of dose rate or dose fractionation.

(2) On this, or any other *nonthreshold assumption*, it follows that even the smallest dose is associated with some risk. Under these circumstances, the exposure of the population to any increase in radiation should not occur unless there is reason to expect some compensatory benefits.

(3) Because of our present limited information, an accurate estimate of the hazard and the benefits of a specific level of radiation is not possible. Therefore, pending more precise information, we *recommend* that the population permissible dose for man-made radiation be based on the average natural background level.

Although it is not our responsibility to determine the exact level, we believe that the population permissible somatic dose from man-made radiations, excluding medical and dental sources, should not be larger than that due to natural background radiation, without a careful examination of the reasons for, and the expected benefits to society from a larger dose.

It is expected that, because of fluctuations in time and location, the population average dose will be considerably less than the maximum permissible dose.

(4) For purposes of computation, it should be permitted to average the amounts over a suitably long period of time, *e.g.* one year, and a reasonable sized population.

(5) For radiation sources, such as radioactive strontium and iodine, which deliver radiation predominantly to one organ or tissue, the maximum permissible dose should be established for the tissue or organ that is expected to receive the most radiation.

(6) It is not possible at present to monitor the population dose solely by measuring the dose to individuals. Furthermore, any effective control over radiation levels must be directed at the levels of radiation and radioactive materials in the environment. This means that maximum permissible levels will need to be established for such factors as food, water, and air. The levels should be set so that the typical person in the area will not receive more than the established permissible dose when all sources are combined.

(7) It is recognized that setting environmental levels involves assumptions and conversion factors to translate these into human body levels. These factors may be expected to change with new information, so the environmental levels may be expected to require continuous revision even though the maximum permissible limits to the body are not changed.

(8) Recommendations regarding a maximum permissible level for medical and dental exposures to the patient are not given because for somatic effects of radiation the possible harm and prospective benefits occur in the same individual in contrast to radiation involving genetic material. The committee urges that continual caution be exercised to maintain radiation for medical and dental purposes at the lowest feasible level.

(9) Finally, the committee wishes to emphasize that under one of the primary assumptions made in this report (nonthreshold linear dose response), the biological effect does not suddenly change from harmless to harmful if any permissible dose is exceeded. Any permissible level which may be chosen is essentially arbitrary and every effort should be made to keep the radiation dosage as far below the permissible level as feasible. On the assumption noted above, any radiation dose should be thought of as being tolerated only to obtain compensatory benefits.

Comments of Drs. John W. Gofman and Arthur R. Tamplin on NCRP Letter

University of California,
Lawrence Radiation Laboratory,
Livermore, Calif., February 25, 1970.

Hon. Edmund S. Muskie,
Subcommittee on Air and Water Pollution,
Committee on Public Works,
U.S. Senate, Washington, D.C.

Dear Senator Muskie: At first we thought we wouldn't even bother to reply to the letter of January 23, 1970 submitted to you by Lauriston Taylor of the National Committee on Radiation Protection. But then we felt that not to reply is to leave this fraudulent, hypocritical, and incompetent document in the record unchallenged.

If this document has, indeed, rceived the approval of the Board of Directors of the NCRP, then the most appropriate response would be the immediate dismissal of President Taylor and every member of that Board. For assuredly this document reflects the utter incompetence and lack of integrity of the ruling board of the NCRP. If such individuals are permitted to continue in such positions, we are making a total mockery of the entire idea of even having a national body involved in radiation protection.

Lauriston Taylor's entire document is filled with emptiness. Not a single, substantive issue is discussed *at all*. Not a shred of evidence supports any of his pious statements that new evidence even points to a lesser risk than previously thought. There is no such evidence.

Dr. Tamplin and I hereby challenge Lauriston Taylor and the entire National Committee on Radiation Protection to a complete debate, including every minute facet of the evidence before a jury of eminent peers who have *no atomic energy* ax to grind, preferably in public view. We have no doubt whatever about the outcome.

The only reason Taylor and the NCRP can see nothing new in what we've presented is that a new idea *has not* entered their heads in the past 20 years. We doubt any new ideas ever will enter their heads.

We *estimate* a 20 times higher risk of cancer than anyone thought possible, and they say nothing is new.

This organization, insofar as Taylor represents it correctly, is in no way devoted to the protection of the health and welfare of the people of the United States. They *are* devoted to protecting technology, at *any* cost to the public health.

Incompetence in the extreme is our only possible evaluation of Lauriston Taylor and his cohorts for the production of this utterly useless report. We await the debate before peers.

Kindest personal regards,

Sincerely,

JOHN W. GOFMAN, M.D.,
ARTHUR R. TAMPLIN.

Federal Radiation Council

FEDERAL RADIATION COUNCIL,
Washington, D.C., December 18, 1969.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: Thank you for your December 1 letter requesting my comments on the statements made by Drs. Gofman and Tamplin during the recent hearings conducted by the Subcommittee.

These statements will receive careful and thorough review, and when the review is completed I will be very pleased to submit the results to you.

Sincerely,

PAUL C. TOMPKINS,
Executive Director.

Department of Health, Education, and Welfare

THE SECRETARY OF HEALTH, EDUCATION, AND WELFARE,
Washington, D.C., January 28, 1970.

Senator EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: This is in reply to your letter of December 1 pertaining to the testimony of Drs. Gofman and Tamplin for the hearings of the Sub-Committee on Air and Water Pollution, on November 18.

Gofman and Tamplin, in reaching their conclusion that the Federal Radiation Council guidelines should be "reduced *now* to 0.017 rads or even less," used an approach similar in principle to that used by expert advisory groups (e.g. ICRP, NCRP, FRC) in developing radiation protection standards and guidelines. This approach is based on the assumption of a direct linear and non-threshold relationship between dose and biological effect. In contrast to Drs. Gofman and Tamplin, however, these expert groups generally agree that this approach probably overestimates the risks, but is the prudent one to use in the formulation of radiation protection guides.

While we concur with this basic approach, we do not agree with all the premises, conditions and extrapolations used by Gofman and Tamplin in their testimony. In general, we believe that their calculations result in overestimates rather than, as they indicate, "minimum values" of cancer risk. Nevertheless, we believe that there is a need to establish more definitive estimates of the radiation risks that are associated with assumed, or observed, exposure conditions; otherwise, there is inadequate basis to evaluate benefit versus risk. We also agree with the concept that the radiation standards should be developed on the assumption that any increase in radiation exposure will be accompanied by a commensurate increase in the risk of cancer.

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Drs. Gofman and Tamplin have raised the question of whether the present FRC guidelines are still acceptable. In the past ten years, since the formulation of the FRC basic guides, sufficient additional information has developed from epidemiologic studies and animal experiments so that a reevaluation of such guidelines is believed to be warranted.

In view of our concern with the potential hazard of ionizing radiation in the environment, and as Chairman of the FRC, I am recommending that the Council institute a careful review and evaluation of the relevant scientific information that has become available in the past decade. I am recommending that this reevaluation provide, as definitively as possible, estimates of the risks associated with low levels of environmental radiation as a basis for review of the adequacy of current FRC guidelines as applicable to projected radiation levels. Based on projected exposure classes of radiation sources, such as nuclear power reactors, other peaceful uses of nuclear energy, and radiation from consumer products would also be considered.

I hope that these comments are useful to your Sub-Committee. Please call on us if we can be of any additional assistance.

Sincerely,

BOB FINCH, *Secretary.*

Atomic Energy Commission

U.S. ATOMIC ENERGY COMMISSION,
Washington, D.C., December 24, 1969.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate

DEAR MR. MUSKIE: Thank you for your letter of December 1, 1969, concerning statements made by Drs. Gofman and Tamplin during the recent hearings of your Subcommittee on Air and Water Pollution. I am enclosing a copy of a report entitled "AEC Staff Comments on Papers and Congressional Testimony by Dr. John W. Gofman and Dr. Arthur R. Tamplin." This material was prepared in response to an earlier request by the Joint Committee on Atomic Energy, and copies have already been provided that Committee. I believe that this report should serve to place the statements of Drs. Gofman and Tamplin in better perspective.

As indicated in my letter of November 28, 1969, we are proceeding with a review of the transcript of the hearings and hope to be able to provide written comment on other testimony in the near future with the thought that you may wish to include it as additional information in the record of the hearings.

Should further information be desired, please let me know.

Cordially,

GLENN SEABORG, *Chairman.*

Enclosure.

AEC STAFF COMMENTS ON PAPERS AND CONGRESSIONAL TESTIMONY BY DR. JOHN W. GOFMAN AND DR. ARTHUR R. TAMPLIN

In a speech before the IEEE Nuclear Science Symposium in San Francisco on October 29, 1969, and in testimony presented before the Senate Subcommittee on Air and Water Pollution, November 18, 1969, Drs. John W. Gofman and Arthur R. Tamplin, Lawrence Radiation Laboratory (Livermore), proposed that the Federal

Radiation Council Guidelines be reduced tenfold, from the presently allowable 170 mrem to 17 mrem per year. In this testimony, Gofman and Tamplin argue that ". . . the already-documented evidence amply justifies a drastic revision *downward*—and now."

Scientists seriously interested in this problem assume a tremendous task of reviewing a vast body of information on biology, physics and mathematics, and dosimetry before they can reach scientifically sound conclusions. Scientists specializing in the field of radiobiology, when speaking as scientists, must be responsible for presenting not only those facts appropriate to a particular thesis, but all of the facts and scientific arguments bearing on the question.

A recommendation to lower the existing standards would appear appropriate only (1) if data have become available that were not considered by the responsible radiation protection bodies (National Council for Radiation Protection and Measurements, Federal Radiation Council, and International Commission for Radiological Protection), or (2) if valid new interpretations and conclusions have been established through recognized scientific channels.

The papers offered by Gofman and Tamplin have been reviewed with these two conditions in mind.

With respect to condition 1, a review of ICRP publications clearly indicates that all of the pertinent information referred to by Gofman and Tamplin has been considered previously and analyzed by the radiation protection bodies as well as additional evidence not considered by Gofman and Tamplin.

With respect to condition 2, the authors' presentation appears to rely on a single assumption; namely, that available data on the incidence of radiation-induced cancer should be interpreted in terms of doubling dose[1] and that "all forms of cancer show closely similar doubling doses and closely similar increases in incidence rate per rad."

The concept of a doubling dose as applied to carcinogenesis by Gofman and Tamplin is here reviewed in detail and found to be without scientific validity.

As an ICRP Task Group pointed out (ICRP, *14*, 58, 1969):

"In radiological protection the radiation dose required to double the natural cancer incidence is sometimes used in assessing acceptable risks for somatic exposure by analogy with the concept of doubling dose used in assessing the genetic risks from exposure of the gonads. This concept of doubling dose for somatic hazards is a specific example of the misuse of the ratio of cancer rates. The natural incidence of stomach cancer in men and women in five different countries varies between 65 and 706 per million living so that for a fixed risk per rad the doubling dose varies more than ten-fold and will induce between 65 to 706 additional cases of stomach cancer depending on the particular population to which attention happens to be drawn . . . the only reasonable parameter to use is the actual number of cases induced by the exposure under consideration."

Not only have Gofman and Tamplin ignored wide differences in the spontaneous incidence rates of different tumors for which they undertook to calculate doubling doses (for lung, breast, stomach, pancreas, bone and other cancers), they have also asserted that their calculations are "*hard, incontrovertible data . . . facts not opinion.*"

Whether they are justified in evaluating hazard by use of the "doubling dose" is a matter of opinion. Constituted National and International bodies have not used the concept except for the case of genetic damage where the dose-response relation is believed to be reasonably linear over a range and variety of radiation exposures. As noted above the ICRP warns using a risk estimate to calculate a corresponding doubling dose. In addition to the reason cited, it is recognized that the currently available human data are not sufficiently precise for confidence in the assumptions that must be made.

Leukemia and thyroid cancer are tumors for which there are no data to allow some useful conclusions to be drawn about the relationship between the radiation dose and the induced incidence of cancer. In the case of leukemia, the data from the Hiroshima and Nagasaki survivors and British patients treated for

[1] The dose required to double the natural incidence of a disease.

ankylosing spondylitis (a form of arthritis) show an approximately linear relationship between dose and induction of leukemia in the range between 100 to 900 rads total dose. Both groups were irradiated at high dose rates. When this reasonably linear relation is extrapolated back to very low doses, it can be calculated that an exposure of 1 million persons to 1 rad may induce a total of 20 cases of leukemia over and above those occurring spontaneously. This is the same as saying that an exposure of 1 million persons to 1 rad may induce one to two additional cases of leukemia per year, since all radiation-induced leukemias will have appeared within 15 years of exposure.

This estimate of risk of leukemia induction at low doses is not a firm scientific fact, but an extrapolation from data obtained above 100 rads delivered at high dose rates. The application of this risk value to a population whose potential exposure is 500 times less than the range of the data and dose rates from environmental exposures over 100,000 times lower is on very weak scientific grounds.

This type of calculation can only be made for tumors on which there are adequate data on a dose relationship and here one must bear in mind that any calculation of risk for doses below that at which the effect has been demonstrated remains hypothetical—certainly the resulting calculated figures are neither "hard" nor "incontrovertible data."

As has been widely recognized, meaningful epidemiological data on radiation induced cancer can be obtained only when these take into account age specific rates[2] of the spontaneous incidence Gofman and Tamplin failed to utilize these data in the Japanese paper that they cited (ABCC-TR-24-68, copy enclosed).

As in the case of leukemia the incidence of thyroid cancer shows approximate proportionality to dose between 100 to 300 rads. With respect to Gofman and Tamplin's considerations of the doubling dose which they suggest is between 5 and 10 rads for younger people, ICRP (Publication No. 8, page 9), states: "Despite variation in the reasons for irradiation, field size, use of anterior or posterior fields, and other factors, the results of these studies are all compatible with a risk of the order of 1 case per million children per rad per year to the thyroid." The ICRP continues: "The best estimates of the total risk (lifetime) that can now be made after allowing for the numbers of cases that are likely to occur in the future on the basis of current knowledge about the distribution of latent periods is that 1 R will produce a total of 10 to 20 cases per million persons. It must be remembered, however, that the carcinogenic effect has been observed in a range of doses of more than 100 rads and these data alone are insufficient to justify attaching any serious credence to the existence of a linear relationship."

Thus, Gofman and Tamplin's estimate of 100 rads as the approximate "doubling dose" for the development of thyroid cancer in the Japanese has no validity. There are simply not enough cases of thyroid carcinomas among irradiated adults in the Japanese study to enable one to obtain a dose-response curve, a necessary prerequisite to determining a doubling dose. Furthermore, Gofman and Tamplin refer to the Japanese data as, "primarily based on adults," when it is known that 50% of those exposed at Nagasaki and 37% of those at Horishima were under 20 years of age at the time of radiation exposure.

Gofman and Tamplin imply that the ABCC studies indicate an approximate doubling of lung cancer incidence rate for 100 rads of exposure or a 1% increase in risk of lung cancer incidence in the population exposed to 1 rad. To quote the authors of the ABCC study referred to by Gofman and Tamplin: "A trend was noted for the risk of lung cancer to increase with increase in exposure dose. By sex, the same trend was noted for males, but for females perhaps due to the fact that the cases totaled only 18, the trend was not definite." Examination of the data offers no support to the 100 rad doubling dose figure since a dose-response relationship has not been established. Furthermore, Miller (*Science, 166*; 569, 1969) has offered other objections.

It is not clear how Tamplin and Gofman obtained their 250-500 rads doubling dose; the reference they cite does *not* give a doubling dose. The basis for applying a two-fold correction for latency is unjustified since it ignores any consideration of age at risk or age of the population. The possibility of a curvilinear relationship (less effect at low dose) between dose and lung cancer

[2] Age specific rates refer to the incidence rates for a defined age group within the population, e.g., children aged 5 to 7 years, adults 50 to 55 years, etc.

(Wagoner, *et al*, N.E.J.M. *273*: 181, 1965) would caution against extrapolation from high doses to low doses in the calculation of a doubling dose.

With regard to the doubling dose of 100 rad for breast cancer based on the Japanese study there is no adequate evidence in the ABCC data to support this estimate. This has been commented by Miller (op. cit.) as follows: "Among the Japanese survivors of the atomic bomb, only leukemia and thyroid cancer have been shown to be radiation-induced. The evidence pertaining to cancer of the breast or lung is still very much in doubt."

To derive doubling doses for other forms of cancer: stomach, pancreas, bone, lymphatic and other blood forming organs and cancers of miscellaneous origin, Gofman and Tamplin have combined their hypothetical doubling dose for cancer of the lung (175 rads) with the data of Court Brown and Doll (paper enclosed) on the number of deaths observed as a proportion of those expected from cancer of heavily irradiated sites (in patients treated for ankylosing spondylitis). The doubling dose concept as here applied to cancer is invalid because:

1. They are not related to the actual doses received by the critical tissues.
2. They were calculated from an entirely hypothetical base.
3. The doubling dose they calculate for stomach cancer directly contradicts the data from the ABCC used partially to develop the doubling dose for lung cancer.

The main body of scientific data for human exposure, primary therapeutic exposures, have not been cited by Gofman and Tamplin; thus, the analysis is biased. The world literature contains data for twelve general classes of human exposures and important supporting animal studies. Epidemiological and cytogenic studies are available for the following groups: (1) radiologists; (2) ankylosing spondylitis irradiation therapy patients; (3) radium therapy patients; (4) uranium and fluorspar miners; (5) patients treated for hyperthyroidism; (6) radium dial painters; (7) patients who received thorotrast; (8) women treated for cervical cancer; (9) thymus irradiation; (10) *in utero* irradiation; (11) accidental radiation exposures, and (12) those exposed at Hiroshima, Nagasaki and the Marshall Islands.

Four major problems are involved in extrapolating available data to the low loses of concern here.

1. Establishment of cause and effect. Leukemia associated with irradiation *in utero* has been shown to be associated with other factors in addition to radiation.
2. Dose rate of all exposures is thousands of times greater than that common to the exposures with which we are concerned.
3. There is evidence for an effective or practical threshhold yet no allowance has been made for levels of radiation below which cancer cannot be causally related.
4. The majority of radiation-associated carcinogenesis data indicate a relation between dose and cancer which shows less and possibly no effect at low doses compared to high doses. These data come from chromosome and animal studies not cited by Gofman and Tamplin. The literature contains about 300 publications on this problem.

Regardless of the shape of the dose-response curve, the only valid scientific approach to describing the possible cause-effect relations and inferring risks therefrom is to establish goodness-of-fit values for various models or hypotheses. Sparse data severely limits, but does not exclude this scientific method.

There is continuing concern supported by organized efforts to understand the quality and quantity of radioactivity dispersed into the environment by man. The national and international committees who recommend permissible limits have in the past and will continue in the future to be advised by competent scientists, and scientific branches of all governments with regard to the hazards of atomic age industry. This continuing conscientious and scholarly review of all pertinent data relating to low level effects forms the basis for the collective opinions and judgments of the major radiation protection groups. This is a healthy and appropriate approach which is to be encouraged. If warranted, the NCRP, ICRP, and FRC will modify standards. However, the opinions and scientifically questionable derivations of Gofman and Tamplin do not make a case for revision of radiation protection standards.

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CARCINOGENESIS IN ATOMIC BOMB SURVIVORS

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INTRODUCTION

It is generally accepted that radiation is an important factor in carcinogenesis, but much remains unknown, such as the mechanism of radiation in carcinogenesis and the relationship of radiation dose and carcinogenesis.

In order to elucidate these unknown points study of carcinogenesis in the survivors of the atomic bomb is very important. This study should make contributions not only in shedding more light on the question of carcinogenesis as a late effect of exposure to atomic bomb radiation but also in providing a scientific basis for the need of preventing radiation effects in the peaceful use of radiation and atomic energy for which there is an increasing demand in industry, science and medicine.

JNIH and ABCC have made a number of joint studies to determine the late effects of atomic bomb exposure. In regard to cancer mortality and morbidity, a study is under way concerning the relationship between radiation dose and carcinogenesis on the basis of the tentative doses recently estimated for the survivors. The mortality data is obtained from the study of cause of death in the JNHI-ABCC Life Span Study (1) of survivors conducted since 1950 and from the Pathology Studies (2) based on autopsy material on the deceased. Morbidity data are derived from the ABCC Leukemia Registry (3) and from the ABCC-JNHI Adult Health Study (4) through regular physical examinations and laboratory tests.

The gamma and neutron exposure dose has been computed for each survivor on the basis of the tentative 1965 air dose estimates (T65I) and the individual shielding configuration.(5) The dose has not been calculated for those exposed under heavy shielding within concrete buildings and air raid shelters because a suitable method of estimation has not yet been developed.

MORTALITY STUDY

This study is made on a fixed population of approximately 100,000 subjects selected from among the exposed survivors of Hiroshima and Nagasaki who were registered in the supplementary schedules of the 1950 National Census, that is, the survey of survivors of the atomic bomb, and the nonexposed or those who were not in the city at the time of the bomb (AT). Every year a *koseki** check is made on this population for confirmation of survival status. For deaths the cause of death is checked and further efforts are made for autopsy procurement in order to confirm the cause of death. The autopsy rate is approximately 40%. First, the results on the relation between A-bomb exposure and cancer mortality obtained from the mortality data will be reported

Table 1 shows the Life Span Study sample by total exposure dose. The survivors number approximately 82,000 and the dose has been computed for all except about 3800 (5%).

To study the relationship between cancer mortality and atomic bomb exposure, it is first necessary to check the accuracy of the causes of death given in the death certificates. Table 2 shows the confirmation rate of cause of death by postmortem examination for the autopsied cases. The confirmation rate is high for all cancers, stomach cancer, uterine cancer, lung cancer, and leukemia, and hence the death certificates giving these diagnoses are highly reliable, but

[1] Hiroshima Branch Laboratory, Japanese National Institute of Health.
[2] Associate Director, ABCC and Director, Hiroshima Branch Laboratory, Japanese National Institute of Health; and ABCC Department of Statistics.
*The official family registration system is based on a permanent address (*honseki*). Changes in address and vital events must be reported to the local office of custody of the records. The record itself is the *koseki*, the office of custody is the *koseki-ka*.

the confirmation rate is about 50% for cancers of the liver and biliary duct, cancer of the pancreas, and cancer of the large bowel, indicating that the death certificates are poor for those diagnoses.

Within the Life Span Study sample there were 2387 deaths from cancer during the 16-year period 1950–66. Figure 1 shows the relationship between the total gamma and neutron dose and cancer mortality for Hiroshima and Nagasaki combined. Subjects are divided into four groups acording to total exposure dose, namely under 10 rad, 10–39 rad, 40–179 rad, and 180 rad or more, shown on the horizontal axis, and the ratio of the observed deaths to the sex and age-corrected expected deaths of each group is presented on the longitudinal axis. The ratio is plotted by the median value of each dose group. The figure shows for the five selected cancers by sex the changes in the risk of cancer death according to dose.

It has been confirmed that in both sexes of leukemia mortality increases markedly with increase of dose. Also, in both sexes for all sites excluding leukemia, a slight trend is noted for the risk to increase with increase in dose. This increment is attributable chiefly to the increase of gastric cancer and lung cancer. Some, for example uterine cancer, show hardly an effect of exposure. A detailed study of the relationship between cancer and dose by site shows a relationship to dose in some instances. For example, for gastric cancer a significant relationship was noted only in the females of Hiroshima, and for lung cancer a significant relationship only in the males of the two cities.

TABLE 1.—EXTENDED LIFESPAN STUDY SAMPLE BY RADIATION (T65) DOSE

Exposure status	T65D (rad) Range	Median	Subjects Hiroshima	Nagasaki
Exposed	0 to 9	0	43,732	11,350
	10 to 39	17	9,622	3,145
	40 to 179	75	5,076	2,484
	180	250	1,603	1,468
	Not estimated		1,901	1,890
Not in city	Early entry		3,712	814
	Late entry		16,497	5,530
Total			82,143	26,681

Table 2—Accuracy of underlying cause of death

Site	*Confirmation rate (in percent)*
All cancer	91
Stomach	83
Uterus, cervix	81
Lung	76
Leukemia	73
Large bowel	53
Pancreas	52
Liver, biliary tract	40

A study was made on the 1576 cases autopsied during 1961–65 within the same study sample. As shown in Figure 2 similar to the aforementioned results of analysis based on death certificates, a trend for higher mortality was noted for gastric cancer (females) and lung cancer (both sexes) in the high dose group, but this is not statistically significant. No significant relationship is noted between exposure dose and mortality due to cancers of the liver and biliary ducts and uterine cancer. Cancers of the urinary system such as cancer of the urinary bladder and cancers of the lymphatic and hematopoietic systems such as malignant lymphoma, as observed for both sexes combined because of the small number of such deaths involved, show higher mortality in the high dose group.

MORBIDITY STUDY

Detailed results have been presented elsewhere on thyroid cancer (7), breast cancer (8), and lung cancer (9) in which radiation effects were noted in recent studies made on the relationship of cancer morbidity and exposure dose using the Adult Health Study sample. These data will be briefly summarized here.

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The Adult Health Study sample is a subsample of approximately 20,000 for Hiroshima and Nagasaki Cities combined which has been selected from the Life Span Study sample. However, as this study, commenced in 1958, has been continued for only about 10 years, it has its weakness for the study of cancer because of the small size of the sample and the short period of observation. However, it also has advantages because a large volume of detailed medical data is available through regular examinations conducted over the past 10 years, such as histories of diseases after A-bomb exposure, abundant clinical and laboratory data, and epidemiological data on various environmental factors other than A-bomb exposure. In this study population a study not possible with the Life Span Study on the relationship between A-bomb exposure and cancers of high survival rate can be conducted.

Thyroid Cancer.—From earlier examination data it is well known that prevalence of thyroid cancer in the proximal exposed is significantly high.(10) Among the approximately 13,000 subjects examined at ABCC during 1964–65, 39 subjects (Hiroshima 25, Nagasaki 14) were found to have thyroid cancer or to have newly developed the malignancy. Figure 3 shows the relationship between total exposure dose and occurrence of thyroid cancer as observed using these data. For females, the risk of thyroid cancer presents a statistically significant increase with increase in dose for those within 2000 m from the hypocenter ATB. A similar trend is observed for males, but because of the limited number of cases it is not so evident as for females.

Further, recent investigation of thyroid carcinoma on the autopsy series in the Life Span Study sample in Hiroshima and Nagasaki has revealed a high incidence of thyroid carcinoma especially of the occult-sclerosing type.(11) Studies are in progress to determine whether this is related to radiation.

Breast Cancer.—Of the 12,000 female subjects of the Adult Health Study in Hiroshima and Nagasaki, 29 breast cancer patients were observed during 1950–66. As seen in Figure 4, the incidence rate of breast cancer by total dose shows a definite increase with increase in exposure dose.

Lung Cancer.—Figure 5 shows the relationship between lung cancer in the Adult Health Study population and exposure dose based on the 1950–66 data. In this sample 66 lung cancer cases were confirmed. The ratio of the observed number of cases to the expected number was compared by dose. A trend was noted for the risk of lung cancer to increase with increase in exposure dose. By sex, the same trend was noted for males, but for females, perhaps due to the fact that the cases totaled only 18, the trend was not definite. Study was also made to determine the relationship between smoking habit, a factor besides radiation considered to elevate the risk of lung cancer, and radiation exposure to lung cancer incidence. It was noted that both smoking and radiation tended to increase the risk of lung cancer. However, because the number was limited detailed studies should be made in the future.

Leukemia Registry.—Lastly, results of analysis will be briefly presented on the leukemia cases recorded up to the end of June 1967 in the Leukemia Registry conducted by ABCC in the two cities.(12)(13) Since leukemia is a disease of low incidence, the relationship between leukemia incidence and total exposure dose was studied using the Master Sample, the largest fixed sample at ABCC as denominator.

Figure 6 shows by city the total exposure doses and the average annual incidence rate of all leukemias during the past 16 years. In Hiroshima a trend is noted for leukemia incidence to increase consistently with increase in dose. However, in Nagasaki, while incidence increased with dose as in Hiroshima for the group exposed to 100 rad or more, no increase was noted under 100 rad. The same trend is noted even when corrected for sex, age ATB, and yearly attrition of the sample due to illness or death.

Figure 7 shows the relative risk for Hiroshima and Nagasaki Cities by the ratio of the observed number to the expected number corrected for sex and yearly attrition of the sample. The data were divided into three dose groups, under 5 rad, 5-99 rad, and 100 rad or more, because a finer division would make the number of cases in each group extremely small. In both cities in the high dose group of 100 rad or more the risk was markedly high in the young age group as compared with the old age group. At the low dose level, such a trend, though not so marked, was observed only in Hiroshima.

Figure 8 shows by city the relative risk of leukemia classified into acute and chronic leukemia, by comparing the ratio of the observed number to the expected number by dose. In survivors exposed to 100 rad or more the risk of acute and chronic leukemia is increased in both cities. However, in the low dose level under 100 rad, while a significant increase in risk is noted in Hiroshima, especially of chronic leukemia, such a trend is not noted in Nagasaki.

It would be difficult to draw a definite conclusion by making further classification of the data and conducting analysis of the various factors because the sample number would become small for comparison of the individual factors. Summarizing these results on the dose response of leukemia, a marked increase in leukemia risk is noted in the survivors of both cities for exposure doses of 400 rad or more, but for those exposed to low doses of under 100 rad, a difference was observed in the dose response between Hiroshima and Nagasaki. This is also evident by type of leukemia.

There is a difference in the sample size between the two cities and further qualitative difference in atomic bomb radiation exists. Radiation was comprised of gamma and neutron rays in Hiroshima while it was mostly gamma rays in Nagasaki. Is the difference in dose response due to the fact that neutrons do not have the same biological effects as gamma rays or is it due to error in the estimated exposure dose? There is an evident need for careful analysis of data in the future.

DISCUSSION

Further with reference to cancers other than those mentioned here, a study is under way for reviewing all medical data accumulated during the past 20 years with regard to exposure dose and risk of cancer for cancer cases in the extended Life Span Study sample for which detailed exposure data are available. Already data are under analysis on malignant lymphoma, thyroid cancer, cancer of the liver, bile ducts, and mutiple tumors. Research protocols for more detailed studies on gastric cancer, lung cancer, breast cancer, and urinary bladder cancer are being prepared or have been proposed to determine the relationship of factors other than atomic bomb exposure to carcinogenesis. For example, how are other environmental factors and radiation exposure related to carcinogenesis, such as smoking to lung cancer and sociological factors to gastric cancer?

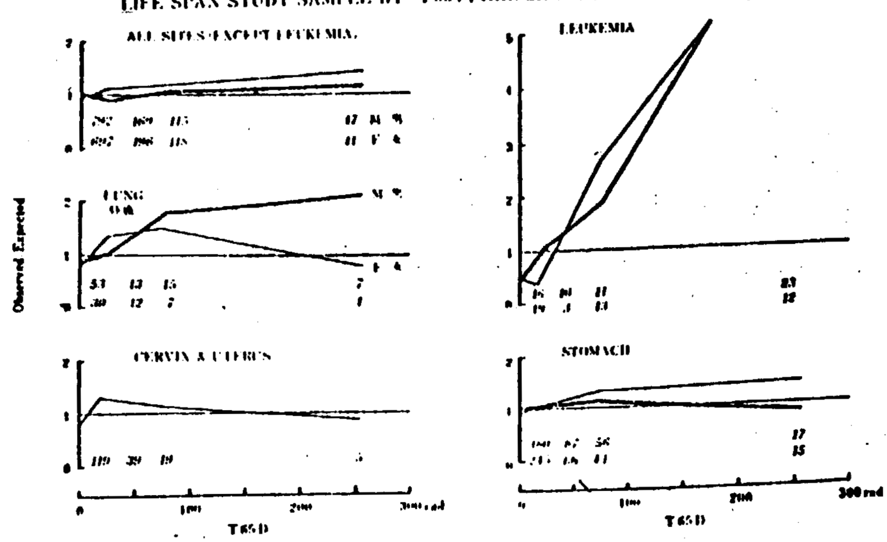
Our studies on the relationship of atomic bomb exposure and carcinogenesis have just begun with much yet unknown: for example, difference in the cancer risk due to the qualitative and quantitative differences in gamma rays and neutron rays between the Hiroshima and Nagasaki bombs and the question of latent period for radiation induced cancers. Increase in leukemia was first noted after atomic bomb exposure, followed by increased risk of thyroid cancer, breast cancer, and lung cancer, and therefore the incidence of cancers of other sites may be increased in the future. Much needs to be clarified through future studies, including the question of disappearance time of carcinogenic effects in atomic bomb survivors. Our studies have many restrictions but in order to provide answers to these unsolved questions it is to be desired, for instance, that all local institutes of research and medical care strongly support the Tumor Registry conducted by the Medical Associations of Hiroshima and Nagasaki Cities for accumulation of more accurate information on cancer.

SUMMARY

The results obtained to date from the various joint studies being conducted by ABCC and the Japanese National Institute of Health to determine the late effects of atomic bomb exposure are summarized with reference to the relationship between ionizing radiation from the bomb and carcinogenesis.

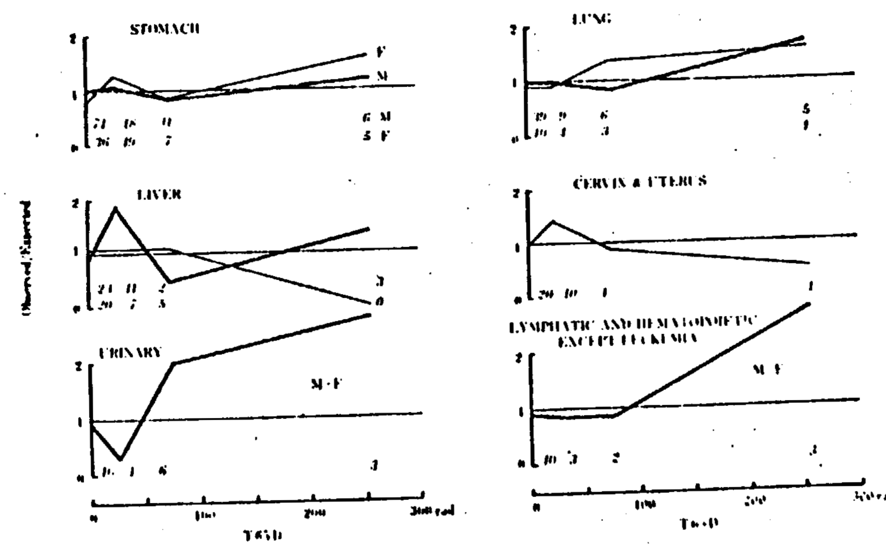
A review is made of the results of studies concerning exposure dose and carcinogenesis on the basis of the newly estimated tentative exposure dose T65D, and on two types of research data, i.e., cancer mortality data and cancer morbidity data. The mortality data were obtained from the study of cause of death in the Life Span Study and from the Pathology Studies based on autopsy material, and the morbidity data from the Leukemia Registry and the Adult Health Study based on clinical studies. The problems and the methodology for clarifying the unsolved questions concerning the relationship between carcinogenesis and atomic bomb exposure are also discussed.

FIGURE 1 OBSERVED/EXPECTED DEATHS ASSIGNED TO CANCER IN THE JNIH-ABCC LIFE SPAN STUDY SAMPLE BY T65D, HIROSHIMA & NAGASAKI 1950-66



Index - number of cases for each dose group

FIGURE 2 OBSERVED/EXPECTED AUTOPSY CANCER CASES IN THE JNIH-ABCC LIFE SPAN STUDY SAMPLE BY T65D, HIROSHIMA & NAGASAKI 1961-66



Index - number of cases for each dose group

FIGURE 3 THYROID CARCINOMA RATE/1000 IN THE ABCC-JNIH ADULT HEALTH STUDY SAMPLE BY SEX & T65D, HIROSHIMA & NAGASAKI 1961-65

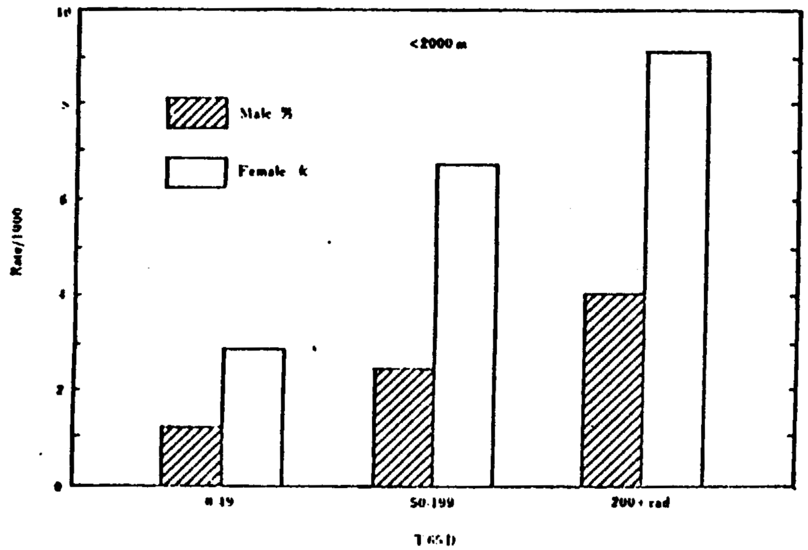


FIGURE 4 BREAST CANCER RATE/1000 IN THE ABCC-JNIH ADULT HEALTH STUDY SAMPLE BY T65D, HIROSHIMA & NAGASAKI 1950-66

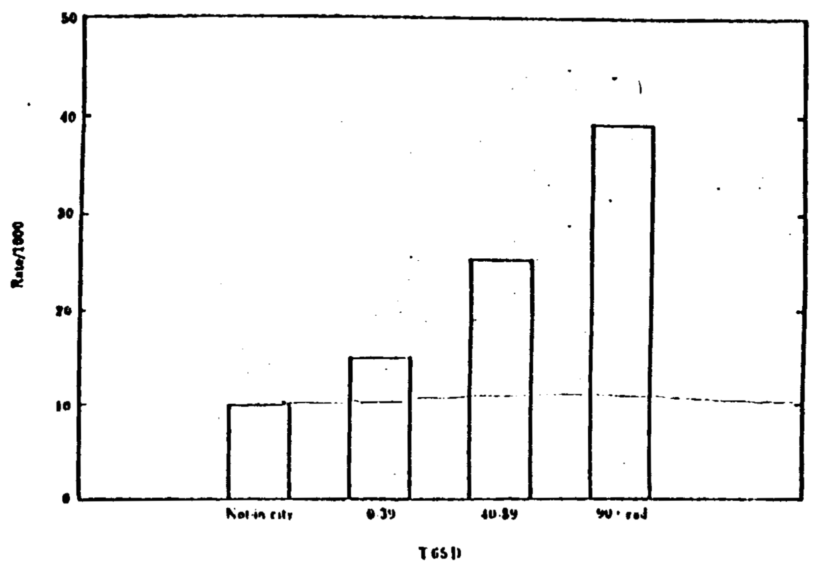


FIGURE 5 OBSERVED/EXPECTED LUNG CANCER CASES IN THE ABCC-JNIH ADULT HEALTH STUDY SAMPLE BY T65D, HIROSHIMA & NAGASAKI 1950-66

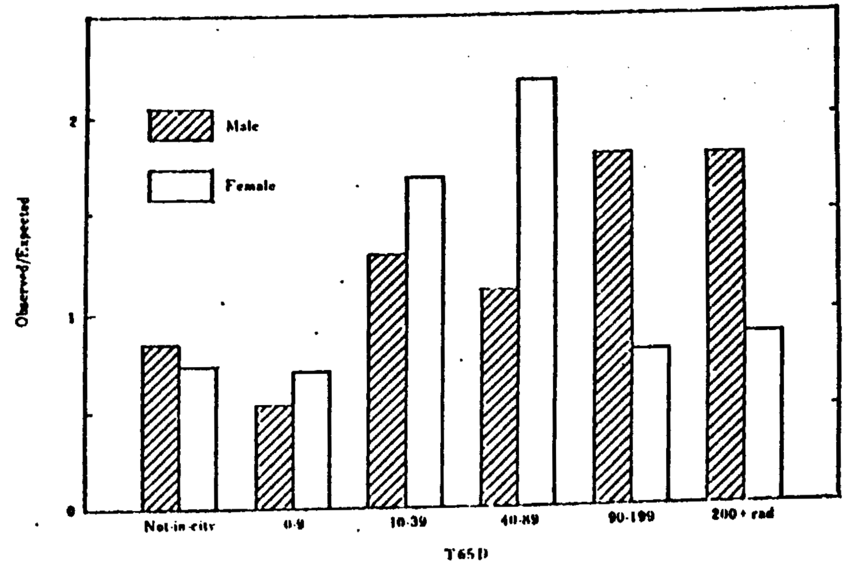


FIGURE 6 INCIDENCE OF DEFINITE OR PROBABLE LEUKEMIA AMONG ATOMIC BOMB SURVIVORS IN THE MASTER SAMPLE BY T65D AND CITY, 1950-66

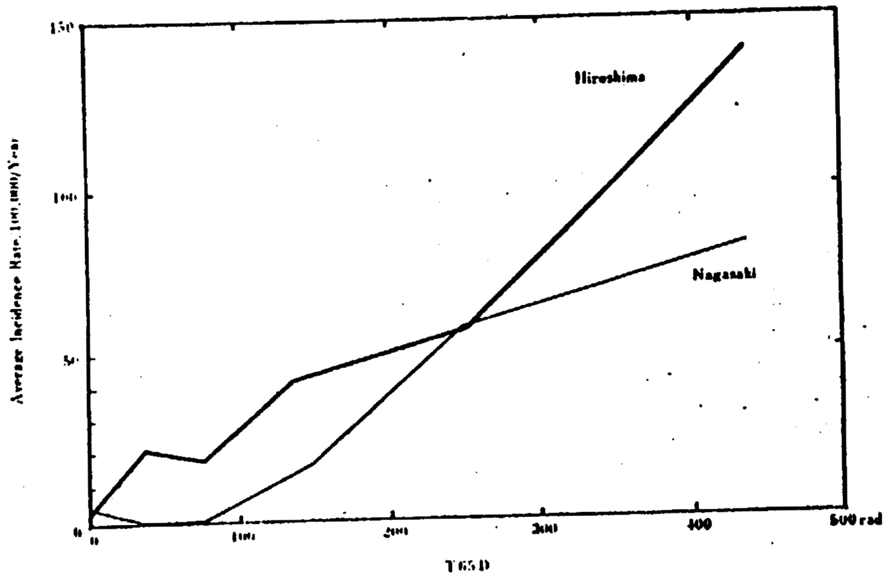


FIGURE 7 RELATIVE RISK OF DEFINITE OR PROBABLE LEUKEMIA AMONG ATOMIC BOMB SURVIVORS IN THE MASTER SAMPLE BY AGE ATB, T65D AND CITY, 1950-66

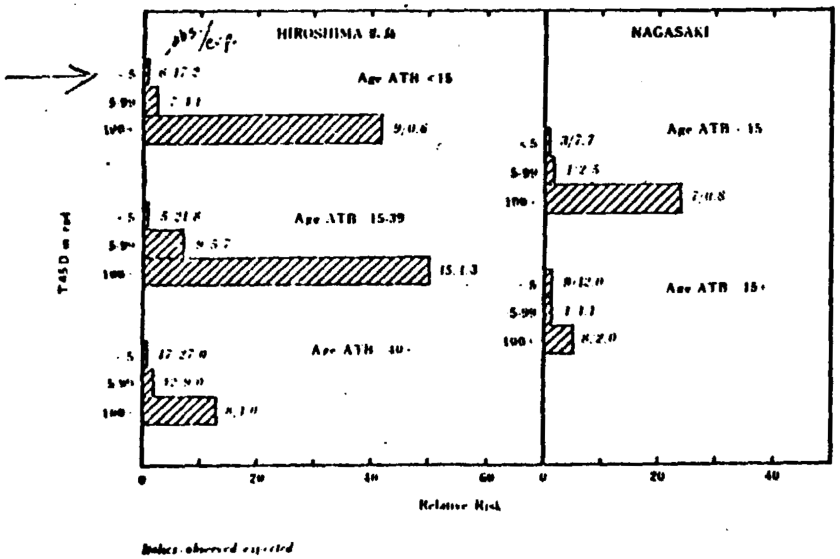
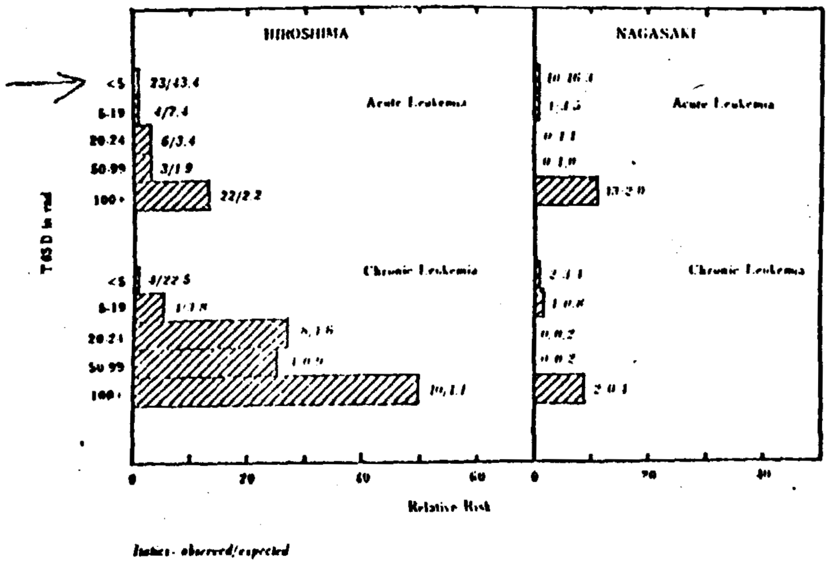


FIGURE 8 RELATIVE RISK OF DEFINITE OR PROBABLE LEUKEMIA AMONG ATOMIC BOMB SURVIVORS IN THE MASTER SAMPLE BY TYPE, T65D AND CITY, 1950-66



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MORTALITY FROM CANCER AND OTHER CAUSES AFTER RADIOTHERAPY FOR ANKYLOSING SPONDYLITIS

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In 1955 the Medical Research Council requested us to investigate the incidence of leukaemia in man following exposure to ionizing radiations, and to examine the nature of the relationship between the dose of radiations and the extent of the incidence of the disease. With the help of a large number of radiotherapists and other colleagues[1] we chose for investigation patients who had been given radiotherapy for ankylosing spondylitis during the period 1935-54, and extracted details relating to them from the records of 81 radiotherapy centres. Estimates of the numbers of patients receiving different doses were obtained from the records of a stratified sample of approximately one in six of all the patients, and the subsequent incidence of leukaemia was determined: (1) from existing follow-up records, (2) by an appeal for information in the medical journals, and (3) by matching the names of the treated patients with a nominal roll of all patients known to have died of leukaemia or aplastic anaemia in the British Isles between 1945 and 1955 (Medical Research Council, 1956; Court Brown and Doll, 1957).

The method used to discover the incidence of leukaemia was chosen because it enabled a result to be obtained quickly. It proved to be highly efficient (see footnote 6), but the lack of a complete follow-up meant that no estimate could be made of mortality rates from other diseases. In particular, no reliable information was obtained about the incidence of other forms of cancer. Other cancers could not be studied in the same way, because so many death were involved, and information about the mortality attributed to these cancers has had to wait until all the patients have been followed individually.

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We now report the results obtained by following the patients until 1 January 1960—that is, over a follow-up period which varies for individual patients from 5 to 25 years. During the follow-up we have had the opportunity of re-examining the radiotherapy records and have added a few patients whose notes had previously been overlooked. We have also been able to make the series more complete by adding material from a few other radiotherapy centres which, for one reason or another, had not been able to co-operate previously; and we have made it more accurate by demonstrationg at follow-up the identity of a few patients whose repeated attendances had been attributed to two or more individuals.

DATA

Patients were included in the study if they had been treated for ankylosing spondylitis (International List No. 722.1) during the years 1935–54 at any one of 87 radiotherapy centres in Great Britain and Northern Ireland. All 76 centres at which radiotherapy was provided by the National Health Services on 28 July 1955 were included, as were 10 which had previously given radiotherapy, but which had ceased to do so, and one which provided radiotherapy privately outside the Service. The co-operation of this last centre was particularly helpful, as it was the first to use radiotherapy for ankylosing spondylitis, and many spondylitic patients who attended at it in the 1930s had been treated with wide-field irradiation to the whole trunk.

Patients were excluded from the series only if it would have been difficult or impossible to obtain the relevant details of their x-ray dosage or subsequent history—that is, if: (1) they had previously been given radiotherapy abroad, in private, or before 1935; (2) they were known to be living abroad at the time of their first treatment; or (3) their radiotherapy notes were missing or grossly incomplete.

There remained 14,554 patients for study.[3] Table I shows the numbers separately for each sex who first came under observation in each year. Table II shows the numbers known to have died or emigrated before 1 January 1960 and the proportion lost to follow-up. It also shows that adequate follow-up information was obtained in more than 98 percent of all cases.

TABLE I.—NUMBER OF PATIENTS STUDIED, BY SEX AND YEAR OF 1ST OBSERVATION

Year of 1st observation	Number of— Men	Women	Total number
1935	27	8	35
1936	30	7	37
1937	53	14	67
1938	82	27	109
1939	116	34	150
1940	112	31	143
1941	136	33	169
1942	253	33	286
1943	436	58	494
1944	630	67	697
1945	698	94	792
1946	804	136	940
1947	790	174	964
1948	987	212	1,199
1949	1,232	233	1,465
1950	1,250	267	1,517
1951	1,171	299	1,470
1952	1,175	241	1,416
1953	1,146	200	1,346
1954	1,033	225	1,258
All years	12,161	2,393	14,554

[3] The precise number of patients excluded is not known as some of their records were destroyed after our initial report (Court Brown and Doll, 1957). It is, however, certainly less than the 432 that we originally reported, as patients treated previously at one large clinic outside the National Health Service have been brought into the study, and the number excluded because of missing or incomplete radiotherapy records has been reduced to 88.

The cause of death certified on the death certificate was obtained for all but two of those who died, and the deaths were classified according to the detailed International List (World Health Organization, 1957).

TABLE II.—STATE OF PATIENTS AT END OF FOLLOWUP PERIOD (JAN. 1, 1960)

State at Jan. 1, 1960	Patients Number	Percentage of total
Alive	12,358	84.9
Emigrated	362	2.5
Died	1,582	10.9
Not known	252	1.7
All States	14,554	100.0

OBSERVATIONS

Expected Mortality

The numbers of deaths that would have occurred if the patients had suffered only normal mortality rates were estimated by multiplying the person-years at risk by the corresponding national mortality rates for England and Wales.

The numbers of person-years at risk were first calculated separately for each sex, each five-year age group, the six-year period 1935–40, and each quinquennium from 1941–5 to 1956–60. Patients were counted as being at risk for half a year in the year in which they first came under observation, and those who died or emigrated were counted as being at risk for half a year in the year in which they ceased to be under observation. Those few patients whose status was not known on 1 January 1960 were assumed to have been still alive. The numbers at risk in each subgroup were then multiplied by the sex and age-specific mortality rates for the corresponding period.[3] For this purpose use was made of rates which had previously been calculated (McKenzie, Case, and Pearson, 1957; Case and Pearson, personal communication) and which covered all causes of death combined, all cancers, the principal separate types of cancer, and the principal respiratory diseases. Mortality rates for other diseases were calculated from data published in the Registrar-General's annual reports. For several of the individual causes of death, however, separate data had been published only since 1949—that is, for ankylosing spondylitis, amyloid disease, ulcerative colitis, non-rheumatic chronic endocarditis, and aplastic anaemia—and for these causes it was assumed that the rates prior to 1951 were equal to those in 1949–50,[4] with a small adjustment to make the sum of all the mortality rates equal to the known mortality from all causes. The error resulting from the lack of complete information for the years before 1949 is, however, negligible, since only 11% of the total deaths occurred during this period.

Comparison of Observed and Expected Mortality

Table III shows that the total death rate among the patients was approximately 1.8 times as high as the corresponding national death rate in England and Wales. The excess mortality was, however, not distributed evenly among all disease groups, but varied from one-tenth of the national rate (cancer of lightly irradiated sites) to nearly 1,000 times the rate (ankylosing spondylitis). Several factors are likely to have contributed to the high mortality, and the disease groups shown in Table III have been divided into four principal classes corresponding to the different types of factor.

Class A includes deaths directly attributed to arthritis and other forms of rheumatism, excluding rheumatic fever. As would be anticipated, the mortality from these causes is greatly increased compared with the national experience. The majority of the deaths were attributed directly to ankylosing spondylitis (89), but a substantial number were attributed to other or less exactly specified rheumatic conditions (52)—that is, rheumatoid arthritis (18), spondylitis not specifically described as ankylosing (6), spinal arthritis (3), osteoarthritis (17),

[3] Rates for the quinquennium 1936–40 were assumed to have held for the period 1935–40.
[4] For amyloidosis and ankylosing spondylitis the rates for 1958–9 had to be used throughout.

and arthritis (8). The occurrence of excess deaths due to these latter causes can be attributed partly to the inclusion of patients with forms of rheumatism other than ankylosing spondylitis in the group of patients originally studied and partly to inaccurate or incomplete certification of the cause of death.

TABLE III.—NUMBER OF DEATHS OBSERVED AND EXPECTED, BY CAUSE

Disease class, cause of death	Number of deaths		Observed deaths divided by expected
	Observed	Expected	
Class A:			
Ankylosing spondylitis	89	0.09	988.9
Other arthritis and rheumatism	52	1.23	42.3
Total	141	1.32	106.8
Class B:			
Amyloid disease	6	.20	30.0
Ulcerative colitis	25	1.32	18.9
Nephritis	47	13.89	3.4
Pulmonary tuberculosis	131	44.66	2.9
Chronic endocarditis, not specified as rheumatic	17	6.02	2.8
Pneumonia	70	27.39	2.6
Other respiratory disease	39	15.35	2.5
Cancer of colon	25	14.78	1.7
Total	360	123.60	2.9
Class C:			
Aplastic anaemia	15	.51	29.4
Leukaemia	52	5.48	9.5
Cancer of heavily irradiated sites	200	127.27	1.6
Total	267	133.25	2.0
Class D:			
Other gastro-intestinal disease	39	16.97	2.3
Peptic ulcer	25	15.06	1.7
Other disease	105	67.10	1.6
Cerebrovascular disease	94	74.98	1.3
Bronchitis	71	56.36	1.3
Violence	86	67.58	1.3
Other circulatory disease	332	257.35	1.3
Cancer of lightly irradiated sites	60	52.42	1.1
Total	812	607.83	1.3
Cause unknown	2		
All classes, all causes	1,582	866.00	1.8

Class B includes deaths attributed to conditions that are known to be clinically associated with ankylosing spondylitis. Non-rheumatic chronic endocarditis and perhaps ulcerative colitis may be attributed directly to the same process that gives rise to spondylitis. Amyloid disease is a secondary complication, and the excess of deaths attributed to nephritis may in fact be due to unrecognized amyloid disease of the kidneys (Cruikshank, personal communication [5]). Pulmonary tuberculosis, pneumonia, and other respiratory diseases, excluding bronchitis, have long been recognized as being associated with ankylosing spondylitis, and their high fatality is presumably related to immobility of the chest. Deaths attributed to these conditions may, however, also include a small number due to a type of upper-lobe fibrosis that is part of the primary pathological process (Campbell and MacDonald, 1965). Cancer of the colon is included in this group because it is an important complication of ulcerative colitis. Much of the colon must, however, have been irradiated from radiotherapy given to the lumbar spine and sacro-iliac joints, so that radiation may also have been a contributory caue. In practice the contribution of ulcerative colitis to the risk of cancer of the colon is unlikely to have been great. If ulcerative colitis occurs normally in 2 per 1,000 of the population and the incidence is increased twentyfold in spondylitis (as is suggested by the increase in mortality), and the risk of colon cancer

[5] Professor B. Cruickshank has reviewed the pathological findings in all patients in the series who came to necropsy, and will be reporting his results separately.

is increased tenfold in subjects with ulcerative colitis, it follows that the risk of colon cancer in spondylitis must be expected to be about 38% greater than normal, and any excess above this might be attributable to other causes.

Class C includes conditions which may be due to the treatment with ionizing radiations, rather than to the original disease. An increased mortality from leukaemia and aplastic anaemia among these patients has been reported previously (Court Brown and Doll, 1957), and further experience has provided very similar results. Both conditions have a high fatality rate, and individual follow-up of all patients has revealed very few cases not already known from mortality records.[6] One patient was found to be alive with leukaemia when the follow-up was completed (1 January 1960), four patients were found to have had leukaemia, and eight to have had "aplastic anaemia," whose deaths were primarily attributed to some other cause. Several of the patients recorded as having had "aplastic anaemia" were, however, found to have had undiagnosed aleukaemic leukaemia (Court Brown and Doll, 1957), and the amount of true aplastic anaemia associated with irradiation must be substantially less than the figures imply. Conversely, the amount of leukaemia must be somewhat more.

Cancer other than leukaemia has not previously been reported as a complication of radiotherapy for ankylosing spondylitis. In the present study the principal types of cancer were divided—without knowledge of the results—into two groups according to whether the organ of origin was or was not likely to have received a substantial amount of radiation from a standard course of radiotherapy to the whole spine and sacro-iliac joints. The brain and central nervous system, mouth, liver and gallbladder, rectum, breast, uterus, prostate, testes, kidneys, and urinary bladder are classified as lightly irradiated sites and all others as heavily irradiated. The division is not always clearcut and leads to some anomalies which could not be avoided. The spinal cord, for example, must have been heavily irradiated. Deaths from tumours of this site are, however, relatively rare, and it is difficult to obtain an estimate of the number expected separately from the number attributed to tumours of the brain, so that the whole group, "brain and other parts of the central nervous system," was classed as being lightly irradiated. Conversely, much of the skin and skeletal system was outside the range of the direct beam of irradiation, and these sites could have been classified as lightly irradiated rather than heavily. In a few centres, moreover, patients had been treated with whole-body irradiation or with "pelvic baths," and in these patients the rectum, uterus, prostate, and urinary bladder will have received doses comparable to those received by other "heavily irradiated" organs. The number of patients treated by these methods is, however, relatively small, so that for the group as a whole these sites can be regarded as having been lightly irradiated.

The relative increase in mortality from cancers in heavily irradiated sites is not large compared with that observed for some other conditions; but the normal mortality from these cancers is high and the excess number of deaths attributed to them (73) is slightly greater than the excess number attributed to leukaemia and aplastic anaemia combined (61).

Class D includes conditions for which the mortality might have been expected to be normal or close to normal. In fact all the conditions studied showed some increase in mortality, and, though the increase was not in general large, the total experience was sufficiently great for the increase of deaths in this group to be statistically highly significant.

Mortality at Different Periods after Irradiation

Further evidence about the reasons for the increased mortality can be obtained by examining the relationship between the date of treatment and the date of appearance of the excess deaths. This is indicated in Table IV. Data are given separately for cancer, aplastic anaemia, and all other causes of deaths combined, and the observed numbers of deaths are shown for three-year periods up to a final period 15 to 24 years after entry (see footnote to Table IV). For the great majority of patients the date on which they came under observation in the series corresponds to the date of first treament. For a few patients, however, there was evidence that radiotherapy had been given previously at some

[6] Only one patient was discovered who developed leukaemia before the end of 1955 and who was overlooked in our previous report. He died of leukaemia in 1948, but this was not recognized owing to a transcription error which led to the belief that he was still alive some time later.

other centre, but the earlier treatment had not brought the patient under observation (either bacause the initial records had not been found or because treatment took place at a center not included in the survey). The proportion of patients who had received radiotherapy before the date of entry (2.4%) is, however, small, and for the present purpose can be ignored.

TABLE IV—NUMBERS OF DEATHS OBSERVED AND EXPECTED, BY CAUSE AND PERIOD AFTER 1ST OBSERVATION

Cause of death	Number of deaths	Years after 1st observation[1] 0 to 2	3 to 5	6 to 8	9 to 11	12 to 14	15 to 24	All periods
Leukemia	Observed	7	19	14	6	5	1	52
	Expected	1.10	1.49	1.32	.86	.45	.27	5.48
Aplastic anaemia	Observed	3	7	4	1	0	0	15
	Expected	.11	.14	.12	.07	.04	.02	.51
Cancer of heavily irradiated sites	Observed	33	36	46	46	27	12	200
	Expected	22.43	33.25	31.32	21.16	11.54	7.52	127.27
Cancer of colon	Observed	6	8	4	5	1	1	25
	Expected	2.94	3.96	3.52	2.33	1.21	.82	14.78
Cancer of lightly irradiated sites	Observed	13	15	13	12	2	5	60
	Expected	10.27	14.09	12.64	8.27	4.28	2.88	52.42
All other causes	Observed	234	336	290	191	113	66	1,230
	Expected	139.07	178.56	155.45	102.30	54.22	35.93	665.56
All causes	Observed	296	421	371	261	148	85	1,582
	Expected	175.97	230.49	204.37	135.99	71.74	47.44	866
Number of person years at risk		35,453	40,746	31,906	19,247	9,558	4,886	141.796

[1] The year after first observation was calculated by subtracting the calendar year in which the patient first came under observation from the calendar year in which he was subsequently observed. Since patients first came under observation, on average, half-way through the calendar year, the first "3-year period" covers, on average, only rwo and a half years—and varies for different individuals from 2 to 3 years. The next four periods each cover 3 years for all individuals, but the actual periods observed vary slightly—for example, for a man who entered on January 1 of any year the second period covers the 4th, 5th, and 6th years and for a man who entered on December 31 it covers the 3d, 4th, and 5th years.

The results show that the pattern of the time-relationship varies for different groups of diseases. For all causes of death other than cancer and aplastic anaemia the morality rate bears a practically constant relationship to the expected mortality, varying only from 1.7 to 1 in the first two and a half years to 2.1 to 1 after 12 to 14 years (Table V). In contrast, the relationships for leukaemia, aplastic anaemia, and cancer of heavily irradiated sites show evidence of a trend with time. For leukaemia and aplastic anaemia the mortality increases between the first and second periods and then falls off, the reduction being more marked for aplastic anaemia than for leukaemia. For cancer of heavily irradiated sites the mortality is at first higher than expected, falls to near normal levels at three to five years after entry, and then rises to more than twice normal 9 to 14 years after entry.

TABLE V.—NUMBER OF OBSERVED DEATHS EXPRESSED AS A PROPORTION OF THE NUMBER EXPECTED, BY CAUSE AND PERIOD AFTER 1ST OBSERVATION

Cause of death	Years after 1st observation 0 to 2	3 to 5	6 to 8	9 to 11	12 to 14	15 to 24	All periods
Leukaemia	6.4	12.8	10.6	7.0	11.1	3.7	9.5
Aplastic anemia	27.3	50.0	33.3	14.3	0	0	29.4
Cancer of heavily irradiated sites	1.5	1.1	1.5	2.2	2.3	1.6	1.6
Cancer of colon	2.0	2.0	1.1	2.1	.8	1.2	1.7
Cancer of lightly irradiated sites	1.3	1.1	1.0	1.5	.5	1.7	1.1
All other causes	1.7	1.9	1.9	1.9	2.1	1.8	1.8
All causes	1.7	1.8	1.8	1.9	2.1	1.8	1.8

The increased mortality from cancer of heavily irradiated sites in the first few years after entry is in all probability an artifact due to misdiagnosis. Several of the patients who died within a year or so from carcinoma of the stomach, pancreas, lung, or prostate had presented with pain in the back, and there is reason to believe that in these patients back pain was produced by tumour involvement of the spine, either by direct spread or by secondary deposits, and was mistaken for that caused by spondylitis. In retrospect it is clear that the presenting symptoms had been due to cancer and not to spondylitis.

The number of deaths more than 15 years after entry is small, and the fall in mortality compared with the previous period is not statistically significant. Further evidence about the nature of the relationship at this period can, however, be obtained by a preliminary examination of the results of prolonging the follow-up period for a further three years to 1 January 1963. Certain knowledge of the status of the patients at this date has been obtained for only 41%, but the annual numbers of deaths recorded in the last three years are not very different from those recorded in the three previous years (1957, 187; 1958, 170; 1959, 178; 1960, 151; 1961, 148; and 1962, 115), and it seems probable that we already know about three-quarters of the deaths that occurred. If, therefore, we assume that all those not known to have died or emigrated were still alive and in the British Isles on 1 January 1963, and recalculate the expected mortality on this basis, our results will be biased by somewhat underestimating the mortality rate among the patients and somewhat (but very slightly) overestimating the expected mortality. The results may, however, give us more useful information about mortality rates more than 15 years after entry, because the relevant numbers of person-years under observation will have been doubled (nearly 10,000 against less than 5,000).

The results given in Table VI now show that the ratio of the observed and expected mortality is practically the same at all periods more than eight years after entry to the study (2.1 to 1, 2.3 to 1, and 2.2 to 1), and that the absolute excess mortality from cancer of heavily irradiated sites continues to increase (1.27 per 1,000 persons per year at 9 to 11 years after entry; 1.69 per 1,000 at 12 to 14 years after entry; and 1.98 per 1,000 at 15 or more years after entry). It may be noted also that, in contrast to the results for cancers of heavily irradiated sites, the provisional results of the extended follow-up confirm the suggestion that the excess mortality from leukaemia and aplastic anaemia had passed its peak within 15 years of coming under observation.

Cancers of lightly irradiated sites show no significant increase in overall mortality, and, as would be expected, provide no evidence of any unusual trend in incidence with the passage of time. Cancer of the colon also shows no definite evidence of a trend, but the number of excess deaths due to this condition is small.

TABLE VI.—COMPARISON BETWEEN NUMBERS OF DEATHS OBSERVED AND EXPECTED, BY SELECTED CAUSES AND PERIOD AFTER FIRST OBSERVATION: INCOMPLETE FOLLOWUP TO JAN. 1, 1963

Cause of death	Years after first observation 0–2	3–5	6–8	9–11	12–14	15–27	All periods
Leukaemia[1]:							
Observed deaths	7	19	16	10	7	1	60
Expected	1.10	1.49	1.59	1.27	0.76	0.54	6.75
Observed as proportion of expected	6.4	12.8	10.1	7.9	9.2	1.9	8.9
Aplastic anaemia[1]:							
Observed deaths	3	7	5	1	0	0	16
Expected	0.11	0.14	0.14	0.11	0.06	0.05	0.61
Observed as proportion of expected	27.3	50.0	35.7	9.1	0.0	0.0	26.2
Cancer of heavily irradiated sites:							
Observed deaths	33	36	52	67	46	35	269
Expected	22.48	33.25	38.55	32.52	20.29	15.67	162.76
Observed as proportion of expected	1.5	1.1	1.3	2.1	2.3	2.2	1.7
Number of man-years at risk	35,453	40,746	37,363	27,082	15,221	9,766	165,631

[1] Although all patients have not been followed individually until January 1, 1963, the total number of deaths is probably known, as the names of the untraced patients have been checked against a nominal role of persons dying of these conditions.

Cancers at Individual Sites

Though there were 200 deaths attributed to cancers of heavily irradiated sites (269 when the follow-up is prolonged to 1 January 1963), the numbers attributed to most of the individual types of cancer are small. It is, however, of some interest to see whether the excess mortality can be wholly explained by an increase in a few specific cancers or whether it is spread widely over a large number. We have already suggested that the increased mortality in the first two and a half years is likely to be an artifact, and we have shown that the observed mortality is very close to the expected mortality three to five years after entry. Clearly, therefore,

we are interested only in the results obtained after six or more years. These are shown in Table VII, both for the period up to 1 January 1960, when follow-up is almost complete, and for the longer period to 1 January 1963, when the results were biased by underestimating the actual deaths and overestimating the number expected. From Table VII it is evident that the greatest absolute excess occurred with cancer of the lung, followed at a considerable distance by a miscellaneous group (mainly carcinomatosis, primary unknown), cancer of the stomach, tumours of the lymphatic and haemopoietic system other than Hodgkin's disease or leukaemia, and cancer of the pancreas. For all these and for cancers of the pharynx and of the bones the observed excess is statistically significant ($P<0.025$ on a one-tailed test). There is, however, very little difference in the ratio of the numbers of deaths observed and expected, which varies only from between 4 and 5 to 1 (cancer of the pharynx and bones) to less than 1 to 1 (cancer of the oesophagus, Hodgkin's disease, and cancer of the skin).

TABLE VII.—NUMBERS OF DEATHS OBSERVED AND EXPECTED FROM CANCER OF HEAVILY IRRADIATED SITES 6 OR MORE YEARS AFTER 1ST OBSERVATION, BY SITE

Primary site (death certification)	Completed followup at Jan. 1, 1960			Incomplete followup at Jan. 1, 1963		
	Number of deaths		Observed as proportion of expected	Number of deaths		Observed as proportion of expected
	Observed	Expected		Observed	Expected	
Pharynx	4	0.70	5.7	5	1.05	[1] 4.8
Esophagus	3	2.25	1.3	3	3.37	.9
Stomach	28	16.03	1.7	38	23.62	[1] 1.6
Pancreas	9	3.78	2.4	12	5.71	[1] 2.1
Larynx	1	1.23	.8	2	1.81	1.1
Bronchi	59	35.65	1.7	96	54.20	[1] 1.8
Ovaries	1	1.44	.7	4	2.16	1.9
Skin	0	.95	0	0	1.37	0
Bones	2	.79	2.5	[2] 5	1.11	[1] 4.5
Hodgkin's disease	1	1.78	.6	1	2.47	.4
Other lymphatic and hemopoietic tissues	7	2.30	3.0	[3] 10	3.39	[1] 2.9
Others	16	4.62	3.5	24	6.78	[1] 3.5
Heavily irradiated sites	131	71.53	1.8	200	107.04	[1] 1.9

[1] Difference between observed and expected statistically significant—$P<0.025$ on a 1-tailed test.
[2] The reliability of the diagnosis of primary bone tumor on a death certificate is not high. Only 3 deaths have been confirmed as due to bone sarcoma; the expected number of deaths is estimated from data given by Mckenzie, Court Brown, Doll, and Sissons (1961) to be 0.63.
[3] 7 cases of lymphosarcoma or reticulosarcoma, one of lymphoma unspecified, and two of myelomatosis. Leukemia is not included in this category (see tables V and VI).

INTERPRETATION

Mortality Associated with Disease Process

The increased mortality from conditions in classes A and B (Table III) can presumably be attributed to the effects of the disease for which the patients were treated. It is possible that some of the increase may be due to the effects of treatment—some cases of cancer of the colon, for example, may have been due to local irradiation—but in the absence of any compelling reason to the contrary we may assume that the abnormal mortality is due to the disease.

The proportionately small but statistically highly significant increase in deaths due to conditions in class D (conditions not grossly related to spondylitis or to irradiation) is more difficult to explain. Several reasons can, however, be suggested.

First, the broad disease groups may contain a small proportion of rare conditions which are in fact directly due to the disease. The large group of "other circulatory diseases," for example contains 43 deaths attributed to "chronic rheumatic heart disease," including seven specifically described as due to lesions confined to the aortic valves. The total number of deaths expected in this category was approximately 22, only one or two of which would normally be attributed to aortic lesions alone, so that some of the excess cardiac mortality—and perhaps a substantial part—may be due to the aortic lesion characteristically associated with spondylitis. Similarly, the deaths due to "other gastro-intestinal disease" include two due to regional enteritis, which bears some relationship to ulcerative colitis and is also associated with spondylitis, and three attributed to non-specific gastro-enteritis or colitis. Violence may also contain some deaths secondarily related to the disease. Spondylitic patients may be more prone to accidents in general, but they are certainly susceptible to a characteristic injury in which the atlas is dislocated and death is caused by pressure of the odontoid process on the medulla (Kellgren, personal communication). Several cases of myelitis and bulbar palsy may have been due to this type of injury, as may some of the deaths attributed to injury in motor crashes.

Secondly, the presence of complications such as amyloid disease and nephritis may lower vitality and increase the risk of death from other unrelated causes.

Thirdly, the cause of death given on the death certificate is not always accurate, and the increased mortality from conditions in classes A, B, and C must be expected to spill over into class D, causing a spurious appearance of increased mortality from genuinely unrelated diseases.

Fourthly, ionizing radiations may have a non-specific deleterious effect—for example, by speeding up the proces of ageing in the irradiated tissues—and they may have caused an occasional death from specific lesions such as radiation myelitis (Boden, 1950; Asscher and Anson, 1962).

Fifthly, other treatments may be harmful. Most patients received several forms of treatment and some deaths may have been related to the use of drugs—peptic ulcer, for example, possibly being due to the use of aspirin, phenylbutazone (Butazolidin), or cortisone.

Sixthly, some of the excess mortality may be an artifact due to the selection of imperfectly comparable death rates for the calculation of the expected mortality. The spondylitic patients may have included a lower proportion of men and women in the Registrar-General's upper social classes than the country as a whole, and the omission of Scottish experience from the national rates will have resulted in a slight underestimate of the expected mortality from some diseases.

Whether radiation produced any non-specific "ageing" effect cannot be determined from these data alone. Certainly the other explanations offered could account for much of the increased mortality; but whether they could account for it all cannot be decided in the absence of detailed information about patients treated by other methods. We are studying a group of spondylitic patients who have not been given radiotherapy, but it has proved difficult to collect a large group who have been followed for a long time, and it will be several years before any substantial results are observed. Meanwhile the constant ratio at all periods after the date of entry into the series between (1) the actual number of deaths from all diseases other than those in class C, and (2) the expected number calculated from national mortality rates, suggests to us that this excess mortality is likely to be dependent on the disease process and unrelated to the form of treatment.

Leukaemia

In contrast to the above, the excess deaths due to conditions in class C bear a clear relationship to the date of irradiation. Much other evidence has implicated ionizing radiations as a cause of leukaemia (see United Nations, 1964), and the new evidence obtained from these observations will not be discussed in detail here. We would add only that: (1) the 60 deaths attributed to leukaemia by the end of 1962 include only one death attributed to chronic lymphatic leukaemia, and in that case the clinical and cytological description proved to be mistaken;[7] (2) review of the evidence confirmed the diagnosis of leukaemia in all cases but one; and (3) some of the cases may be attributable to repeat courses of radiation given some years after the initial course which brought the patient under observation.

The possibility that a drug, or the disease itself, may play some part in the production of leukaemia has to be considered; but, in the light of all the other evidence and the time-relations observed in the present study, we see no reason to doubt that the excess deaths due to leukaemia (less three in which leukaemia was present when radiotherapy was started[8]) were due to the treatment—that is, 44 deaths by the end of 1959 and 49 by the end of 1962.[9] To these should be added

[7] Court Brown and Doll (1957), Case 1.
[8] Court Brown and Doll (1957), Cases 15, 33, and 34.
[9] The one death in which the presence of leukaemia was not confirmed has been subtracted.

some of the excess deaths attributed to aplastic anemia, many of which were unrecognized cases of aleukaemic leukaemia—particularly those occurring more than three years after the start of treatment. In all, the total leukaemia mortality attributable to radiotherapy has been of the order of 60 per 15,000 persons, or 4 per 1,000 over a follow-up period averaging 13 years from the date of first observation. This estimate will presumably be increased with more prolonged observations, but the incidence of new cases appears to be falling off after 15 years, and it may not be increased much above the present figure.

Other Types of Cancer

Less evidence is available in relation to other types of cancer. Several individual types are known to be capable of induction by large amounts of radiation—in particular carcinoma of the pharynx, bronchi, and skin, sarcoma of the bones and soft tissues and endothelioma of the liver. Exposure to moderate amounts of radiation in childhood has produced cancer of the thyroid and it seems probable that exposure to small amounts of the order of 1–10 rads *in utero* produces all the principal types of childhood cancer. Mortality rates from all cancers other than leukaemia were raised in American radiologists compared with those in specialist physicians and opthalmologists and otorhinolaryngologists (Seltser and Sartwell, 1965), and both the mortality and the morbidity rates from cancer other than leukaemia are slightly raised among the heavily exposed survivors of the Hiroshima and Nagasaki explosions compared with the rates among the distant survivors who were effectively not exposed to ionizing radiations at all (Jablon, Ishida, and Yamasaki, 1963; Harada, Ide, Ishida, and Troup, 1963). A clear increase in the prevalence of lymphomas other than leukaemia among the heavily exposed survivors has also been demonstrated (Anderson and Ishida, 1964).

In the present study two observations suggest that the increased mortality rate from cancer is largely an effect of radiotherapy. First, the increase is greater for cancer of sites that have certainly been fairly heavily irradiated than for cancer of other sites, many of which must have received little or no irradiation from the standard course of treatment. Secondly, the time relations between the appearance of the increase and the date of treatment are those that would be expected on clinical grounds if the two were causally related—that is, excess deaths began to appear about six to eight years after treatment and subsequently increased in incidence with the passage of time until 15 years after treatment or longer. The fact that the increase is widely spread among different types of cancer is also an indication that it is due to a non-specific carcinogenic factor. An increase in bronchial carcinoma alone, for example, might have been due to a tendency for spondylitic patients to smoke more (if such a tendency were shown to exist). There is, however, no known factor other than radiotherapy which could account for an increase in so many different types. The possibility that ankylosing spondylitis may create an increased susceptibility to cancer induction must be considered, but it seems unlikely in view of the two specific observations that have been referred to previously. It remains a possibility, however, until a sufficient series of observations have been made on patients treated by other methods.

On present evidence we conclude that the increased mortality from cancers of heavily irradiated sites (other than cancer of the colon) that appeared more than five years after irradiation is due to the treatment. We estimate, therefore, that the excess mortality, attributable to irradiation is of the order of 90 per 15,000 patients or 6 per 1,000 over a follow-up period averaging 13 years from the date of first observation. Contrary to the position with regard to leukaemia, however, there is no evidence to suggest that the main impact of the effect is yet passed, and the mortality attributable to radiations may increase two-fold or more with the further passage of time.

SUMMARY

A total of 14,554 patients with ankylosing spondylitis, who were treated with *x* rays during the period 1935–54, have been studied. More than 98% were traced on or after 1 January 1960, and less complete follow-up information is available for a further three years.

The effects of irradiation have been assessed by comparing the numbers of deaths observed with the numbers that would have been expected if the patients had suffered the death rates recorded in the population of England and Wales as a whole. The most important finding, apart from the previously reported excess of deaths from leukaemia and aplastic anaemia, relates to other cancers originating in heavily irradiated tissues. Deaths attributed to these cancers were increased approximately twofold six or more years after first treatment, and 15 years after first treatment the excess showed no sign of diminishing. The excess was not limited to one or two types of cancer, but many different types contributed to it, approximately in proportion to their normal incidence. In contrast to these findings the number of deaths from cancers originating in lightly irradiated tissues was not increased significantly.

It is estimated that in an average follow-up period of 13 years after first treatment the excess deaths from leukaemia and from other cancers arising in heavily irradiated tissues, which can be attributed to the effects of ionizing radiations, were 4 per 1,000 patients and 6 per 1,000 patients respectively.

Deaths ascribed to spondylitis or rheumatism or to the direct complications of spondylitis were increased, as were to a less extent deaths due to a variety of other causes. Many different factors probably contributed to this, but their importance cannot be finally evaluated until results are obtained from a similar study of spondylitic patients treated by other means.

We would like again to accord our gratitude to the directors and staffs of the 87 British radiotherapy departments who co-operated in this study. We are also greatly indebted to Miss F. Callaby, Miss A. Fotheringham, and Miss K. Jones for the arduous work of following up so many of the patients; to Miss F. Callaby and to Mr. K. Majai for assistance in the analysis of the results; and to Miss M. Devine, of the Medical Research Council's Computer Services Group, who prepared programmes for the most laborious calculations to be carried out on computers.

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Department of Commerce

U.S. DEPARTMENT OF COMMERCE,
OFFICE OF THE SECRETARY,
Washington, D.C., December 12, 1969.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR MR. CHAIRMAN: Secretary Stans has asked me to acknowledge your letter of December 1 (received today) requesting our comments and evaluation of a recommendation by Dr. John W. Gofman and Dr. Arthur R. Tamplin in hearings before your Subcommittee.

We will give you our comments in this regard as soon as possible.

Sincerely,

SOL MOSHER,
Special Assistant to the Secretary.

THE SECRETARY OF COMMERCE,
Washington, D.C., January 16, 1970.

HON. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR MR. CHAIRMAN: This letter is in reply to your letter of December 1 requesting our comments and evaluation of a recommendation by Dr. John W. Gofman and Dr. Arthur R. Tamplin of the University of California given in hearings before your Subcommittee on November 18, 1969. The testimony was entitled, "Federal Radiation Council Guidelines for Radiation Exposure of the Population-at-Large—Protection or Disaster?".

The testimony of Dr. Gofman and Dr. Tamplin raises very serious and important questions concerning the permissible levels of radiation exposure to the population-at-large. The resolution of such questions must be based on a careful evaluation of the data presented by Dr. Gofman and Dr. Tamplin, as well as other relevant scientific data by experts, largely with biological and medical competence. The Department of Commerce, in particular the National Bureau of Standards, has high technical competence in the measurement of radiation and in operations research and stands ready to provide such help and guidance as may be required for the protection of the population. Biological and medical expertise is located elsewhere in the Government, and we are not in a position to make an independent evaluation of the claims of Dr. Gofman and Dr. Tamplin. We feel that the Federal Radiation Council with the assistance of appropriate independent organizations, such as the National Council on Radiation Protection and Measurements, is the proper organization to undertake a careful review of this matter and decide whether or not permissible exposure levels need to be changed. The Department of Commerce, as a member of the Federal Radiation Council, is participating in such an evaluation.

Sincerely,

MAURICE H. STANS,
Secretary of Commerce.

Department of Agriculture

DEPARTMENT OF AGRICULTURE,
OFFICE OF THE SECRETARY,
Washington, December 24, 1969.

HON. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate

DEAR SENATOR MUSKIE: In response to your request of December 1, a review has been made of the statement presented by Drs. Gofman and Tamplin during hearings before the Subcommittee on Air and Water Pollution.

The Department of Agriculture, as a member of the Federal Radiation Council (FRC), participates in the establishment of guidance to the President on matters concerning the public health and safety where ionizing radiation is involved. We have participated in the studies on the scientific basis for the standards recommended by FRC.

It has long been established that ionizing radiation under appropriate circumstances and in sufficient quantities is capable of causing damage and even death to the persons exposed. As a result of extensive studies over a period of some 30 years by those with an interest in radiation protection problems, such as the International Committee on Radiation Protection (ICRP), the National Committee on Radiation Protection (NCRP), and others comprised of experts from Federal government, scientific and technical societies, medical schools, private organizations, and industries, FRC recommended the use of 0.17 rem for yearly whole-body exposure of average population groups. This value is in essential agreement with the recommendations of ICRP and NCRP. The philosophy behind the basic recommendations of NCRP is expressed in Section 4.1 of National Bureau of Standards Handbook 59, Permissible Dose from External Sources of Ionizing Radiation:

"As a matter of principle it is sound to avoid all unnecessary exposure to ionizing radiation, because it is desirable not to depart from the natural conditions under which man has developed by evolutionary processes. However, man has always lived in a field of ionizing radiation due to the presence of radioactive material in the earth and the cosmic rays. Whether exposure to this level of radiation is beneficial or deleterious to man (and the race) is a matter of speculation. The obvious fact is that it cannot be avoided, and it is therefore normal for man to live in this environment.

"We have a lower limit of continuous exposure to radiation that is (unavoidably) tolerated by man. There is, on the other hand, a much higher level of exposure that is definitely known to be harmful. Between these two extremes there is a level of exposure that experience to date shows to be safe for the individual concerned; however, the time of observation of large numbers of people exposed at this rate under controlled conditions is too short to permit a categorical assertion to this effect.

"It should be noted in this connection that lowering the level of exposure by a factor of two or even ten does not materially alter the situation insofar as making a positive statement of absolute safety is concerned. The only statement that can be made at the present time about the lifetime exposure of persons to penetrating radiation at a permissible level considerably higher than the background radiation level, but within the range of radiological experience, is that appreciable injury manifestible in the lifetime of the individual is extremely unlikely. It is therefore necessary to assume that any practical limit of exposure that may be set up today will involve some risk of possible harm. The problem then is to make this risk so small that it is readily acceptable to the average individual; that is, to make the risk essentially the same as is present in ordinary occupations not involving exposure to radiation."

In discussing dose to persons outside of controlled areas, NCRP recommended that radiation or radioactive material outside the controlled area and attributable to normal operations within the controlled area shall be such that it is improbable that any individual will receive a dose of more than 0.5 rem in any one year from such radiation. The operations within the controlled area will normally entail control of the average concentration in air and water. The regulatory controls imposed on industry concerning the release of radionuclides in the environment have been exhaustively studied prior to the decision being made and standards established. Staff reports prepared by FRC contain background material for the development of radiation protection standards.

Based on studies such as those cited above and available related literature, it appears that deleterious effects resulting from application of FRC recommendations have been insignificant. The Department of Agriculture concurs in the findings and recommendations of FRC and this acceptance is further based on the premise that all committees working on this problem operate on the philosophy that the radiation dose to the general public should be kept as low as reasonably possible.

As more quantitative information concerning genetic and somatic effects in both animals and humans is accumulated, exposure limits can be appropriately reviewed. For this reason, it is desirable that scientists like Drs. Gofman and Tamplin perform independent studies as described in their report; however, we are unable to evaluate the medical testimony presented by them due to our unfamiliarity with the basic information from which their conclusions were drawn. Such studies should assist the various organizations in establishing lower radiation exposure limits to the general public if the data presented and properly analyzed so warrant.

Sincerely,

NED D. BAYLEY,
Director of Science and Education.

Comments of John W. Gofman, M.D.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., January 19, 1970.

HON. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution,
U.S. Senate,
Washington, D.C.

DEAR SENATOR MUSKIE: I am replying to your note of January 9th which requested comments on two items:

(a) The answer to you from Department of Agriculture.
(b) The answer to you from Chairman Seaborg for the AEC.

(a) The Agriculture Response

Perhaps the best and only comment is to agree with Dr. Ned D. Bayley of Agriculture when he admits "we are unable to evaluate the medical testimony presented by them due to our unfamiliarity with the basic information from which their conclusions are drawn".

(b) The AEC Response by Chairman Seaborg

I know Glenn Seaborg very well. He guided my Ph.D. thesis. I cannot believe this AEC Staff Report can possibly reflect any of his *scientific* thinking. It is impossible for me to believe that Glenn Seaborg truly subscribes to the rubbish which is represented in the AEC Staff Document. I, personally, would cringe at the thought of submitting that nonsense to an eminent Senator of the United States.

As you know, we are steadily producing a series of additional *scientific* documents. They have been going forward to you as part of the Federal Radiation Council Review of Standards, which is in existence through your Subcommittee's initiative.

As you will see by perusing these documents, they answer scientifically, and point by point, the issues concerning which the AEC Staff Document, to put it charitably, has produced a mountain of empty, trivial words.

Much, much more is coming. What we told your Subcommittee is sound! I think, however, we weren't quite pessimistic enough. The more refined our calculations become, the more serious the radiation hazard demonstrates itself to be.

With kindest regards.

Sincerely yours,

JOHN W. GOFMAN, M.D.

UNIVERSITY OF CALIFORNIA,
BIO-MEDICAL DIVISION, LAWRENCE RADIATION LABORATORY.
Livermore, Calif., February 24, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate, Washington, D.C.

DEAR SENATOR MUSKIE: As I review many of the questions, answers, and comments of the recent Hearings of your Subcommittee, it occurs to me that you and your colleagues may find some things puzzling. Since I have had a great deal of experience working with the people involved in atomic energy and in setting standards, I thought I might be of some help to you to explain the "puzzling" matters. In the words of Chairman Seaborg, when he sent you the AEC Staff Comments on our testimony, I shall give you "some perspective" on the various people who have objected to what we told you or what some others told you about hazards of underground nuclear explosives.

First of all I have to explain how the Plowshare enthusiasts view you and your colleagues of the Senate Subcommittee on Air and Water Pollution. Prior to you people raising questions, the Plowshare enthusiasts always had everything their own way, and such nonsense as public health and safety were effectively set aside in favor of the glory of "technological" progress. And then you and your colleagues came on the scene and asked a whole host of embarrassing questions, mostly pertaining to the preservation of humans on earth. This the advocates of Plowshare technology regarded as an unmitigated nuisance. No one else had troubled them with these embarrassing questions before, and why must they put up with such nonsense now? You see, they had whole laboratory groups working on biological hazards, their reports were duly filed, the authors were patted on the head knowingly, and absolutely nothing happened. All was well.

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And, of course, the last thing to worry about was that any of the biological scientists within AEC would step outside the bounds of "propriety" and speak out critically. The reason this rarely, if ever, happens is that scientists have families, they have mortgages on their homes, and hence they have fear. As a result, a great deal is *known*, but very little is *said* in the "wrong places". In this sort of work, one quickly comes to learn what propriety is, what the boundaries are, and what the dire results of stepping outside these boundaries may be.

As Dr. Tamplin and I studied the problems of hazards to man associated with nuclear energy in more and more depth, we saw many more problems of serious nature than we had earlier anticipated. Aside from the pat on the head I spoke of above, we might as well have been talking into a 100-mile an hour gale. Dr. Tamplin began to present some of his findings publicly, and the AEC was indeed disturbed. Finally some of their staff specifically requested of him in writing and of both of us by phone that we, in effect, provide a "whitewash" of some of the potential hazards of radiation. We refused, in perhaps not the best of Webster's words, especially me. And, as I mentioned above, this sort of intransigeant behavior was essentially unheard of in the AEC "family".

I became more directly involved in the actual hazard calculations, and the results were even more shocking than our worst fears. We presented those calculations at the Institute for Electrical and Electronic Engineers on October 29, 1969—and you might imagine the dismay in the AEC "family", *both* in our laboratory and in Washington Headquarters. You heard about our presentation and invited us to testify before your Subcommittee, and then all the fun began.

We were "urged" not to testify before your Subcommittee. "After all", it was said, "Senator Muskie doesn't understand atomic energy—your testimony will just confuse matters". Or, "Senator Muskie will just use the information politically". Or, "Don't you realize what you may be doing to the budget?"

So we testified before you, and told you the truth as we saw it. Not a pretty picture.

Now to explain some of the reactions since the testimony.

Perhaps you may tend to think harshly of some of the atomic energy proponents you've encountered. They really don't mean to be evil; it's just that they are a bit confused about where technology and people intersect. To them, you, Senator Muskie, just don't understand the situation. You want to do things to make the earth livable for humans, to improve the quality of life. And your colleagues on the Senate Subcommittee want to do the same. But to the Gung-ho atomic energy proponents, all of you fail to understand the glories of technology.

The atomic energy proponents, particularly the Plowshare advocates, are products of weapons research. They like to refer to themselves as "mission-oriented". No matter what "mission" is assigned to them, or they assign themselves, their objective is "mission accomplished". All obstacles *must* be overcome with such "mission-oriented" people, working for the greater glory of "technological progress". And with such an orientation the loss of some human lives is totally secondary. It is not that they are hard-hearted, but as some of them tell me, "People have to die sometime".

or, "Why worry about radiation killing people prematurely—it will save them from dying later".

or, "Everything you do has *some* risk".

or, "So you calculate 16,000 deaths from radiation cancer—that's not as many as automobiles kill each year".

or, as one of the directors of the Livermore Lab tells me,

"I guess it's all right for you to calculate how many thousands of people will be killed by radiation from "peaceful atom" projects. But it's *not* your right to say that 16,000 or 32,000 extra deaths is unacceptable—that's just your opinion, and you should keep it to yourself".

You see, Senator Muskie, it is perfectly acceptable for the directors of the Livermore Laboratory to advocate, promote, and advertise the wonders of natural gas exploration by nuclear explosives. Such advertising they call a "normal laboratory function".

But for me or Dr. Tamplin to comment on the deaths to be caused in the public who buys and breathes the poison gas in their homes—that was just an "opinion" best reserved to ourselves.

In our early work on this radiation hazard question, and when we thought the cancer risk was 20 times smaller, we used to tell the laboratory directors about our concern, and they would listen and say that was interesting. But Plowshare

was not near as a practical reality, so they didn't feel threatened by our idle calculations.

Now, Plowshare projects are near realization. And, concomitantly our recent researches led to our discovery that *all* forms of cancer are radiation-induced—which makes the projected number of deaths 20 times as high—that's what leads to our projection of 32,000 extra cancers plus leukemias from FRC Guideline radiation.

And the Plowshare advocates in our laboratory are furious with us—*and with you and your Subcommittee*. Just when their "mission" was going to come into full bloom, an interloper like Senator Muskie has to come upon the scene and raise questions! And, as for us, members of the "family" to *answer* those questions—this is the last straw!

Homicide is illegal in the United States, so I guess we are at least protected from *overt* homicide. However, lesser reprisals of great effectiveness are quite legal, and I frankly doubt we'll escape those for very long—unless we "get the message". I do have a little additional protection in that I hold a professorship in the University of California. It is extremely easy to understand why you'll have trouble getting information from scientists in AEC labs, or in comparable technology organizations. I can assure you that being a "leper" in your own laboratory, with other scientists carefully looking in several directions before talking to you, is not a pleasant matter.

Furthermore, it's the easiest thing in the world to find scientists who will testify *for* the advantage of the technology. The rewards and the praise are excellent. Many items are purchasable; men *are no exception*.

Senator Muskie, I wouldn't want you to be shocked by any of this. It is just that there are two kinds of people—those who are concerned about humans, the environment, and happiness, and those who are concerned about dollars and technological progress. And as you have heard and must appreciate, variety in people is the spice of life—and death.

Next, with respect to standard-setting, you have asked me with perplexity how our data squares up with Dr. Tompkins of FRC stating that new evidence over recent years even suggests we could *raise* the standards, not lower them. You will have difficulty with this until you understand that there are two possible orientations for people involved in setting standards. The first orientation is to protect the public health, and to resolve all doubts in favor of the public health. The second orientation is that which favors the technology and *always* finds a justification for it. Two justifications always are available:

(a) The benefits of technology are wonderful.

(b) The cost of protecting the public is prohibitive.

Dr. Tompkins has grown up in the FRC with the second orientation. He is extremely sincere in his worries about the cost in dollars of protecting the public health. He had very little to say about the substance of our hazard estimates, but he immediately gave a public interview expressing his fear that "Gofman and Tamplin may *price* society out of business".

I wish you well in your efforts to save the environment for life, health, and happiness. I am not optimistic that you'll get much help from scientists in the "establishment". And, by the way, Universities are in the "establishment", for the refusal of a research grant is a potent silencer. Occasionally, you'll find a couple of half-wits like Tamplin and me, and you'll get a little information. But most scientists know the rules of propriety much better than we do.

The only suggestion I have if you really want to succeed in your efforts is to work with your colleagues to create some "insulated" institutes of 10 or 15 scientists who can work and report their work *without fear of reprisal*. The economic cost of this would be a mere pitance—the results would be fantastic—and might even really make your efforts successful. But could you get your colleagues to help you do this? It's a new idea, you know, really "free scientific inquiry".

Kindest regards,

Sincerely yours,

JOHN W. GOFMAN, M.D.

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APPENDIX III

(The following materials were presented as testimony by John W. Gofman and Arthur R. Tamplin at hearings of the Joint Committee on Atomic Energy, U.S. Congress, Jan. 28, 1970:)

Contents:

A Proposal for at Least a Ten-Fold Reduction in the FRC Guidelines for Radiation Exposure to the Population-at-Large Supportive Evidence

(Oral Testimony by John W. Gofman and Arthur R. Tamplin, Bio-Medical Research Division, Lawrence Radiation Laboratory (Livermore) and Division of Medical Physics (Berkeley), University of California)

[This is Document: Summary Testimony Previously issued: Nos. 3, 4, 6, 7, 1–A, 8, 9, 2–A, 10.]

SUMMARY TESTIMONY

On October 29, 1969, my colleague, Dr. Arthur Tamplin, and I presented, at a scientific meeting of the Institute for Electrical and Electronic Engineers an estimate that exposure of the U.S. population to

F.R.C. Guidelines of 0.17 Rads per year would lead to 16,000 extra cases of cancer annually in the U.S.A. Today, the only way in which we might change that number is to increase it materially, since abundant evidence we have recently uncovered indicates very strongly that the true situation is even worse.

As a result of our scientific presentation, followed by testimony before the Senate Subcommittee on Air and Water Pollution, we have been attacked as "unfriendly to atomic energy." I have been with atomic energy work since long before an Atomic Energy Commission existed, and what is more my contributions in this field with respect to the National Defense are quite clear from the record. I share with this Committee the desire to use atomic energy in any way possible to raise the standard of living of the people of the U.S.A., to achieve any of its benefits—provided we know the risks and make a full disclosure to the public of the true magnitude of the risks. Where controversy or uncertainty exists, we must state publicly the range of our uncertainty. The most potent enemy of atomic energy development is *not* truth, but is false optimism and an ostrich-like approach of refusal to examine the possible risks in a reasonable fashion. The history of the Joint Committee on Atomic Energy has been to achieve the fullest statement of truth in the radiation exposure problem. And that is why the published Hearings of this Committee represent *the* major Scientific Forum for presentation of the issues.

My colleague, Dr. Arthur Tamplin, and I have prepared 9 scientific documents for this Hearing, and you have them before you. We have addressed several of the crucial issues in great detail in these documents. We believe we have several major new developments to report to you in these documents, including perhaps some major new scientific contributions of consequence to medicine and to solving the radiation—risk problem with an accelerated pace not previously thought possible. I shall highlight the subjects covered in these documents; detailed study must be at your leisure.

I. The Question of Safe Radiation Thresholds for Human Exposure

The Document (No. 3) entitled "Studies of Radium-Exposed Humans II" presents our detailed refutation of Professor Robley Evans latest claims that a threshold exists. While this claim has been clearly refused in your previous Hearings, an A.E.C. Staff Document still claims it. The I.C.R.P., Parker, Archer, Morgan, and Snyder all reject the Evans' claims. We have presented in this document detailed analysis of why we do too.

Moreover, in your excellent Hearings on the Uranium Miners, Dr. Evans answered Congressman Hosmer as follows:

(Quote) "I am perfectly glad to turn the statement around the other way. I believe, in a positive sense, that 1-to-3 working levels and a total accumulation of 300 to 400 working level months is innocuous to man".

As you will see from our Document (No. 4) on Uranium Miners, based upon evidence available at the time of your Hearings, we show clearly that a *great deal* of cancer would have been expected at 300–400 WLM.

And now, some two years later, an abundant excess of lung cancer has occurred and been reported by Lundin and Archer in the region of 120 to 360 WLM. In fact, a *four* fold increase in lung cancer is reported. So the threshold concept of Evans predicted safety where in fact a disaster occurred.

Indeed, when anyone involved in setting standards even asks the question of possible safe threshold values, he is courting disaster, for safe thresholds can't, in general, be proved. Assuming they exist leads to the fiasco I just described for the Uranium Miners.

And of the greatest importance, Dr. Stewart in England has just published evidence of a dose-response relationship in the region of 1–5 Rads.

II. The Uranium Miner Story

In the document before you (No. 4) we have proved the fallacy of the doctrine of a "special" form of lung cancer being induced by radiation. We have demonstrated that *two* kinds of lung cancer are clearly induced by radon daughters.

Further, we have provided you with evidence in this document that even the new Uranium Miner Guidelines can lead to serious trouble because of the nature of the problem of exposure (Fully detailed in the document No. 4).

Because of the failure of others to see the two forms of lung cancer induced in the miners, they have falsely cast doubt on lung cancer being radiation induced in Hiroshima-Nagasaki.

III. The Breast Cancer Story

In Document No. 6 we demonstrate to you that breast cancer is clearly induced by radiation in A-bomb survivors as it is clearly proven by Mackenzie to be induced by fluoroscopic radiation in tuberculosis patients in Nova Scotia, Canada. The results are in *excellent* harmony, and, discouragingly, both studies point to 50 Rads or less being enough to double the incidence of breast cancer in two areas, under vastly different circumstances, 7500 miles apart.

IV. The Lung Cancer Story

In Document No. 7 we have reviewed the Hiroshima-Nagasaki Lung Cancer data, previously called into question by Miller and by Storer on grounds we showed in our Uranium Miner paper to be indefensible. The Japanese data are sound and they agree with the Court Brown-Doll British data. The doubling dose is estimated, on careful analysis, to be under 100 Rads for lung cancer in man.

Now, we would like to present to you some major new developments of scientific importance, of human importance, and of importance in grossly simplifying the acquisition of knowledge concerning human carcinogenesis by radiation—more than 100 times as easily as in Hiroshima-Nagasaki. These are also in Document 7. The story is as follows:

1. Steinitz, in a beautifully-executed epidemiological study in Israel proved conclusively that lung cancer is 5–10 times as frequent in people previously treated for tuberculosis. She suspected the tuberculosis itself increases the risk of later lung cancer. The increase itself is enormous—as large as the excess risk of lung cancer in cigarette smokers. We do *not* think tuberculosis, itself, causes this lung cancer.

Rather, we announce to you our hypothesis as follows:

The excessive (5–10 fold) lung cancer in pulmonary tuberculosis cases is due to fluoroscopic radiation associated primarily with pneumothorax treatment in the past, and we suggest—

(a) Immediate world-wide examination of records of tuberculosis patients, including the number of fluoroscopies they received, for their subsequent occurrence or non-occurrence of pulmonary cancer.

(b) Based upon our analysis of Mackenzie's breast cancer study, we predict the average patient in hospitals where treatment was similar to Nova Scotia must have received about 450 Rads of radiation, and this should lead right to the 5–10 fold increase in lung cancer observed by Steinitz. We are confident the study of records of tuberculosis hospitals will confirm all our predictions.

(c) Since millions of people, world-wide, were treated for tuberculosis in 1930–1950, the latent period for lung cancer or breast cancer development is over. There need be no waiting; if the epidemiologic studies are started now, we should have answers pouring in from all over the world in less than a year. And the cases available should be 100 times the extremely valuable Hiroshima-Nagasaki studies. By ranking patients by *number* of fluoroscopies, we may also very well be able to have the entire dose-response curve over a wide rang of radiation doses.

We urge *immediate* initiation of these studies by National Cancer Institute, U.S. Public Health Service, and by other countries. We predict that within one year *few* people will still be debating whether radiation causes lung cancer, breast cancer, and other cancers in the fluoroscopic beam.

(d) If we are right, this may be of the greatest importance in future reduction of cancer in tuberculous persons. New techniques of fluoroscopy can and should be used to reduce dosage, and thereby reduce the risk of lung cancer.

V. Lack of Protection by Slow Delivery (Fractionation) of Dosage

Many people, ourselves included, have *hoped* that slow delivery of radiation, so-called fractionation of dose, will operate to reduce the risk of future cancer. In Document 1A we have presented for you our analysis which indicates, we believe, very strongly that this is simply an illusion, and any idea that the animal experiments in this regard prove "repair" of carcinogenic damage has no foundation whatever. The illusion of protection by fractionation arises from experiments where the *acute* dose is delivered at an earlier period of life, when sensitivity to radiation is, in general, high whereas the protracted dose is continued into a later period of life when sensitivity is lower.

So, instead of proving anything at all that should lead us to be hopeful that slow delivery of radiation lessens the risk of cancer, this study indicates radiation, delivered slow or fast, produces just what is expected from the age at which radiation occurs. Further, this study should make us especially wary of irradiating children.

We are, therefore, extremely pessimistic that there remains much hope for protection from low dose rate.

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Additionally these studies enable us to understand why alpha particles, neutrons, or other so-called high LET radiation are more effective in carcinogenesis than x-rays, α-rays, or β-particles.

VI. Direct Proof of the Validity of the Doubling-Dose Concept

An I.C.R.P. Task Force, with the A.E.C. Staff following close behind, has essentially ridiculed the idea that the doubling dose concept could operate for two groups who differ widely in their expected cancer rate without radiation. In essence what they say is that if one population of individuals has a cancer rate of 70 per million and another population a rate of 700 per million, they can't believe a doubling dose, say 100 Rads, could produce 70 extra cases in the first group and 700 in the second group. They suggest that if 70 extra cases are produced in one population, 70, not 700 will be found in the second population.

Two recent publications, *with direct evidence*, prove the doubling dose concept to be in harmony with observations, while the I.C.R.P. Task Force suggestion leads to incorrect answers. The first study, in Document No. 9, by Lundin, Archer and co-workers is on radiation (from radon daughters) in cigarette smokers and nonsmokers. Without radiation, the cigarette smokers have 10 times the lung cancer incidence of the non-smokers. When they receive radiation to the same degree, in direct contradiction of I.C.R.P. Task Force and A.E.C. staff, the cigarette smokers get 10 times as much cancer from the same radiation dose than do the nonsmokers. Further in an entirely analogous study with asbestos (instead of radiation) plus cigarette smoking, the same asbestos exposure produces *much more* lung cancer in cigarette smokers than in non-smokers.

The doubling-dose concept *is* sound!

VII. The I.C.R.P. Estimates and the Gofman-Tamplin Estimates

Subsequent to our testimony before the Senate Sub-Committee on Air and Water Pollution we have obtained a copy of I.C.R.P. Publication 14. We have reviewed this document and compared it with our testimony. We find that there is substantial agreement between I.C.R.P. Publication 14 and our testimony (See Document No. 8). I.C.R.P has now *drastically increased* its estimate of cancers of diverse organs induced by radiation. Now there is essential harmony between our previous estimates of expected cases of cancers from F.R.C. Guideline exposure and those which would be calculated from I.C.R.P. 14. The one remaining major difference resulted from an oversight on the part of the authors of I.C.R.P. Publication 14. Nevertheless, in spite of their oversight, they concur with us and indicate that the data suggest that the 0.170 rem/year guideline should be reduced by at least a factor of ten.

Thus, the International Commission on Radiological Protection is now in harmony with our estimates of the cancer risk from radiation.

VIII. Radiation-Induction of Cancer in Experimental Animals

An A.E.C. Staff Document makes the following remarkably unbelievable statement,

"The majority of radiation-associated carcinogenesis data indicate a relation between dose and cancer which shows less and possibly no

effect at low doses compared to high doses. These data come from chromosome and animal studies not cited by Gofman and Tamplin".

In Document No. 10 before you, we have presented an analysis of the beautiful and exhaustive experiments of Bond and co-workers on radiation-induction of breast cancer in rats. These studies represent a bulwark of support for the linear hypothesis of radiation induction of cancer, and they point strongly against *any* suggestion of a safe radiation threshold. We concur with Bond and co-workers, and further an analysis of their data indicates an extremely low doubling dose for breast cancer induction in the rat, *between 5 and 20 rads!* These experimental animal data are in close harmony with our calculations (Document No. 6) for human breast cancer.

It is incredible to us that the A.E.C. Staff issues a statement diametrically opposed to the results of one of the finest experiments ever supported by the Division of Biology and Medicine of the A.E.C.

The other part of the A.E.C. Staff statement deals with radiation effects upon chromosomes. Since the field of low dose radiation, chromosomes, and cancer happens to have been my major personal research field for over four years, I feel especially qualified to speak on this subject.

I can say unequivocally, and without fear of contradiction, that there is not one shred of scientific evidence linking the type of chromosome alterations referred to by the A.E.C. with the development of cancer. Moreover, my colleagues and I have presented the evidence concerning which chromosome changes *are* related to cancer in three separate meetings and in documents to the A.E.C. I shall be pleased to provide these documents to this Committee as a supplement if you desire them. Perhaps the A.E.C. Staff does not take our work seriously. I might point out that by unanimous vote of the Program Committee, my colleagues and I have been invited to present an invitational lecture on "Chromosomes and Cancer" at the forthcoming 10th International Cancer Congress in Houston, Texas in May, 1970. And this invitation came long before we had said anything about radiation standards.

TECHNICAL AND PROFESSIONAL QUALIFICATIONS—JOHN W. GOFMAN, M.D., PH. D.

EDUCATION

B.A.—Oberlin College (1939), Chemistry.
Ph. D.—Univ. of California, Berkeley (1943), Nuclear/Physical Chemistry.
M.D.—Univ. of California San Francisco (1946), Medicine.
Internship—Univ. of California Hospital, San Francisco (1946–1947), Medicine.

POSITIONS

1. Group Leader (Plutonium Project) LRL, Berkeley, Calif., 1941–1943.
2. Professor of Medical Physics, University of California, Berkeley (Assistant Professor and Associate Professor up to 1954), 1947–present.
3. Medical Director, Lawrence Radiation Laboratory (Livermore), 1953–1958.
4. Research Associate, Lawrence Radiation Laboratory (Berkeley and Livermore), 1947–present.
5. Director, Bio-Medical Division, Lawrence Radiation Laboratory, Livermore, 1963–1966.
6. Associate Director, Lawrence Radiation Laboratory, Livermore, 1963–1969.
7. Executive Committee on Biophysics, Medical Physics, and Bio-Radiology, University of California, Berkeley, 1960–1964.

8. Reactor Safeguard Committee Member, University of California, Berkeley, 1955–1960.
9. Physician in Radioisotope Therapy, Donner Clinic, University of California, Berkeley, 1947–1951.

SCIENTIFIC NATIONAL CONTRIBUTIONS

1. Co-discovery of U^{232}, Pa^{232}, U^{233}, Pa^{233}.
2. Co-discovery of Slow and Fast Neutron Fissionability of U^{233}.
3. Co-inventor Uranyl Acetate Process for Plutonium Separation.
4. Co-inventor Columbum Oxide Process for Plutonium Separation.

SCIENTIFIC WORK IN ATOMIC ENERGY (ADDITIONAL TO ABOVE)

1. Over 20 years of University teaching in the *radioisotope, radiobiology field* (graduate plus undergraduate).
2. Guidance of many Ph. D. candidates for degrees in Biophysics and Medical Physics.
3. Membership on the Nerva Scientific Advisory Board.
4. Research on Low Dose radiation, chromosomes and cancer.

SCIENTIFIC PUBLICATIONS

Approximately 130 scientific publications, in toto, plus additional reports. 3 Books (additional).

CoEditor, "Advances in Biological and Medical Physics", 1963, 1964, 1965, 1966, 1967, 1968, 1969, and currently.

Studies of Radium-Exposed Humans II: Further Refutation of the R. D. Evans' Claim That "the Linear, Non-Threshold Model of Human Radiation Carcinogenesis Is Incorrect"

(By John W. Gofman and Arthur R. Tamplin)

[This is Document No. 3 in a series of 7. No previous ones issued.]

INTRODUCTION

We have recently refuted Evans' claim that the "linear, non-threshold model of human radiation carcinogenesis is incorrect". Our refutation was based upon an analysis using residual radium burden in the Evans and Hasterlik series of cases. (1) (2) (3)

Recently Evans has published a paper indicating that he prefers analysis to rest upon cumulative rads or cumulative rad-years to the skeleton instead of upon residual radium burden (4). We like and accept Dr. Evans' suggestion that cumulative rad-dose represents a better basis for analysis than does residual radium burden. Therefore, this report is an analysis of all the Evans' observations on radium-exposed persons based upon cumulative rads of exposure.

As the analysis presented below will justify, we state the following:

1. We reject the Evans' claims even more strongly than before.
2. The linear, non-threshold model of human radiation carcinogenesis from 0 cumulative rads through 50,000 cumulative rads is in excellent accord with the observations.
3. The linear, non-threshold model proves acceptable even for doses 10,000 times those relevant for setting FRC Guidelines.

PRELIMINARY DISCUSSION

Why This Report Is Required

The sophisticated worker in this field of human radiobiology may wonder why we should address ourselves at all to the analysis of the data concerning the radium-exposed humans. This does require an answer here.

Such important and highly competent bodies as the International Commission on Radiological Protection have, for years, rejected the Evans' claims of "safe thresholds" and "non-linearity" as being unsupportable by the evidence he has presented. Further, in the important JCAE Hearings "Radiation Exposure of the Uranium Miners", several eminent workers in this field rejected Evans' claims most effectively, including Morgan (10), Snyder (11), Archer (12), and Parker (13). It seemed hardly credible that after such effective refutation, the matter might still come up again for necessary refutation. But it has.

After the convincing serial refutations of Evans referenced above, Evans has recently produced a paper (4) which indicates that if a more appropriate description of dose is used, such as cumulative rads or cumulative rad-years, then the evidence strongly supports his contentions. We have demonstrated here that this is in *no way* true and that all the Evans' claims are totally indefensible scientifically. When a man who has contributed so effectively and extensively, as has Robley Evans, states that on a new basis of calculation, the story is different, it is imperative that his claims be carefully considered and analyzed. It was possible his new interpretation might be correct. But, by careful analysis, we are now convinced it is erroneous.

There exists, however, an even more compelling reason why the radium-exposed human story must be laid to rest for what it is. Recently we presented evidence indicating that radiation carcinogenesis in humans, in all probability, includes *all* forms of cancer rather than certain rare cancers or leukemia. Further, we suggested that each rad contributes a fractional increase in risk of cancer—a fraction of the spontaneous occurrence rate of a particular form of cancer. This, of course, grossly magnifies the extent of the radiation hazard. Our generalizations concerning human carcinogenesis will ultimately stand or fall on scientific merits. As we study the problem in progressively increasing depth, we feel more confident than ever that our generalizations will stand, with only minor revisions at most.

The Division of Biology and Medicine (AEC) has, in our opinion, erroneously interpreted our presentations as an attack upon atomic energy in general and upon the Atomic Energy Commission, in particular. We were surprised beyond belief by this reaction of the Division of Biology and Medicine, for we had fully expected them to welcome our findings as potentially important inputs to a scientific dialogue of the greatest portents for human health of this and future generations. In what we regard as a rather poor effort to discredit the "Gofman-Tamplin findings", the Division of Biology and Medicine has recently "exhumed" the radium story as definitive proof of a "safe practical threshold" of human radiation exposure. We are dismayed to see this occur. We would expect that the ICRP is even more dismayed, but they will surely speak for themselves. Let us quote from two recent documents, widely circulated by DBM–AEC, which herald the "safe threshold" concept. The first comes from Dr. John Storer, a former Associate Director of DBM, who stated the following in a critique he wrote of the Gofman-Tamplin work (14). We quote Storer directly:

"For example, in the case of induction of bone tumors in the radium dial painters, the conclusion should be inescapable that there is an effective dose below which tumors do not appear".

Dr. Storer, with DBM, stands opposed to the following: Morgan, Snyder, Archer, Parker, the ICRP, Gofman and Tamplin.

The second is in a recent DBM–AEC Staff Report, also widely circulated (including Associated Press dispatches). (15) Quoted directly:

"There is evidence for an effective or practical threshold yet no allowance has been made for levels of radiation below which cancer cannot be causally related".

While it is difficult to make sense out of this statement from the DBM Staff Paper, it is meant, in all likelihood, to refer to Evans' "demonstration" of an effective or practical threshold.

Clearly, the Division of Biology and Medicine of the AEC didn't accept all the prior, effective refutations of the Evans' claims. They are widely circulating these claims as proof of safe radiation thresholds, in an apparent effort to influence the forthcoming Federal Radiation Council Review of Radiation Exposure Standards. DBM–AEC should be encouraged to challenge *scientifically any statements we make*. But we fear that recourse to a widely discredited hypothesis derived from radium exposures may, unfortunately, have the effect of diminishing the weight given to any substantive arguments that DBM may have forthcoming. We hope this will not be the case.

So, because Evans has recently refurbished his own hypothesis and because DBM has "exhumed" the same hypothesis as being "inescapable", it has become necessary for us to address ourselves again to this important area. That is why this detailed analytical report has been written.

The Linear Hypothesis and the Use of the Radium Studies at all

As we have stated in the text, there are numerous reservations one should have for drawing *any* conclusions from the studies of radium-exposed persons. Every epidemiologist knows the treacherous pitfalls of using human data where an appreciable fraction of the population under study is lost to analysis. Overt biases can be pin-pointed in such situations; the hidden ones are even more to be feared. (See Archer (12)) The MIT studies of radium-exposed persons is a classic in the annals of epidomiology of the situation where drastic biases may be present. It represents a great contribution to many aspects of human radiation exposure, but we doubt very much that it ever should have been used *at all* for epidemiologic purposes. The ICRP shares this view, we believe. Any capable epidemiologist examining the clinical material available would probably reach the same conclusion.

Why then should one even bother to go through the labor of testing the linear hypothesis on such a poor epidomiological sample. Part of the answer lies in the fact that it is available. *Fortunately*, epidemiological material in this area is only available through the ignorance or unfortunate, often rash, actions of man and society. If some of the crude outlines of dose-response curves can be ascertained through the study of this material, one can assuredly temper conclusions with an ever-present awareness of the epidemiological pitfalls.

Part of the answer lies in the fact that, overtly and covertly, the MIT studies have been used to justify permissible radiation doses as "safe". Even with the epidemiologic pitfalls, it is therefore valuable to be able to demonstrate within the data themselves that certain interpretations made and widely advertised are not correct.

While we have shown here that the linear hypothesis fits the MIT data over a range from 0 to 50,000 rads, we would be wary about overextending the meaning of our findings. We *are* dealing with treacherous epidemiologic material. Our analysis of a 25 rad doubling dose being good from 0 through 50,000 rads does not exclude a gently curving dose-response curve. The doubling dose may be even lower than 25 rads in the higher dosage ranges until finally the "overkill" region is reached at extreme doses where the doubling dose increases *above* 25 rads. People *can* develop multiple cancers, but they can only die once. Since we are dealing with a fatal disease, bone sarcoma (and a glance at Robley Evans' plots shows this), we know that linear theory must break down somewhere in the high dose range. When people get to 30–100% incidence rates of fatal bone sarcoma, this disease alone, or this one plus some of the other fatal diseases, preclude(s) further response with still further increase in dose in the very high ranges. Therefore, at extreme dosages, where everyone gets killed, the doubling dose can rise steeply, even approaching infinity. We would hope, fervently, that no one will be so rash as to misinterpret an "infinite" doubling dose at 50,000 to 100,000 rads as evidence for a safe radiation threshold in that dose region.

THE RADIUM DIAL PAINTERS AND IATROGENIC CASES

Evans has divided his cases into two large subgroups:

(a) The radium dial-painters plus iatrogenic cases

(b) The Chemists exposed to radium.

The circumstances of acquisition of radium burden differ for the two groups, so that Evans' division of the cases into these subgroups is reasonable. We shall analyze both groups of cases starting with group

(a) The radium dial painters plus iatrogenic cases.

In Table 3 (Reference 4), Evans presents the various exposure groups below 1200 cumulative rads as groups (a) through (f). In Table 5 (Reference 4) Evans presents the various subjects suitable for epidemiological appraisal as Class 1–5, ranging from cumulative rad values of 1200 through 50,000. We shall, therefore, analyze all these groups (a through f of Table 3 and Class 1–5 of Table 5) since we believe Evans' subdivisions are quite reasonable and appropriate.

Two kinds of cancers have occurred in the radium-exposed persons, bone sarcomas and carcinomas of the (sinuses plus mastoid). We shall address ourselves, appropriately, to each type of cancer separately in detail.

TABLE 1.—AGE SPECIFIC ANNUAL DEATH RATES FOR MALIGNANT NEOPLASMS OF BONE,[1] U.S. VITAL STATISTICS (1966–67)

Age span (years)	Males: Population at risk (thousands)	Males: Deaths	Males: Death rate (cases per 100,000)	Females: Population at risk (thousands)	Females: Deaths	Females: Death rate (cases per 100,000)
20 to 24	6,625	58	0.83	6,981	16	0.23
25 to 29	5,632	24	.43	5,840	10	.17
30 to 34	5,326	10	.19	5,527	14	.25
35 to 39	5,717	15	.26	5,987	17	.28
40 to 44	6,021	24	.40	6,371	18	.28
45 to 49	5,633	46	.82	5,978	32	.54
50 to 54	5,189	52	1.00	5,498	46	.84
55 to 59	4,490	93	2.07	4,839	63	1.30
60 to 64	3,757	110	2.93	4,174	52	1.25
65 to 69	2,901	107	3.69	3,476	84	2.42
70 to 74	2,261	98	4.33	2,929	92	3.14
75 to 79	1,564	81	5.18	2,124	75	3.53
80 to 84	847	66	7.79	1,230	53	4.31

[1] This category is what we shall, in table 2 and in the text, refer to as bone sarcomas.

TABLE 2.—AGE SPECIFIC ANNUAL DEATH RATES OF BONE SARCOMAS IN U.S. MALES, FEMALES, AND AVERAGE OF MALES AND FEMALES

Age span (years)	Age specific annual death rates, in cases/100,000		
	Males	Females	Average [1]
20 to 24	0.83	0.23	0.53
25 to 29	.43	.17	.30
30 to 34	.19	.25	.22
35 to 39	.26	.28	.27
40 to 44	.40	.28	.32
45 to 49	.82	.54	.68
50 to 54	1.00	.84	.92
55 to 59	2.07	1.30	1.69
60 to 64	2.93	1.25	2.09
65 to 69	3.69	2.42	3.06
70 to 74	4.33	3.14	3.74
75 to 79	5.18	3.53	4.36
80 to 84	7.79	4.31	6.05

[1] Males plus females, divided by 2.

We shall explore whether the linear hypothesis, namely a constant doubling dose for radiation-induction of cancer, is consistent with all the Evans' data, or whether his claimed "threshold" is required.

An analysis of this problem requires several inputs:

1. The expected spontaneous incidence of bone sarcoma and of (sinus+mastoid) carcinoma.

2. The expected radiation-induced cancer incidence.

Item 1. the expected spontaneous incidence of bone cancers can be obtained from U.S. Vital Statistics data.

Item 2. the radiation-induced cancer can be estimated as follows:

(1) We shall use the linear hypothesis that the risk of cancer (per rad) is the same for very low doses all the way up to 50,000 cumulative rads (5). Since this covers a range up to 10,000 times as large as is relevant for FRC Guidelines, this will provide a drastic challenge for the linear hypothesis. Evans says the linear hypothesis is incorrect. By our *drastic* test, we shall give him every benefit in evaluation.

(2) We shall, over this entire range up to 50,000 rads, use 25 cumulative rads as the doubling dose for bone sarcoma or for cancer of the nasal sinuses and mastoid, or a 4% increase in risk of radiation-induction of cancer per rad. In our published "laws" of radiation-carcinogenesis we suggested 1% per rad, but indicated it might well be higher (5) (6). Our reason for using 4% here is partly to credit the widely-held concept that high LET radiation is more damaging than low LET radiation.

Item 1: The Expected Spontaneous Incidence of Bone Sarcoma and of (Sinus+Mastoid) Carcinoma

Two forms of cancer are at issue here. The first is bone sarcoma. The second is carcinoma of the sinuses and mastoid. From U.S. Vital Statistics data the appropriate values for bone sarcoma are available and are presented in Table 1. It will be noted that the values for males are slightly higher than for females. For the calculations below we shall average the male and female incidence rates, as shown in Table 2. Evans does not present the male and female data separately, and this is why we average them. It is our impression that the radium

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dial painters had more females than males in the group, so our u[se] of the average tends to favor the Evans' hypothesis. We are, at ever[y] step, trying to shade all uncertainties *in favor* of Evans' hypothesi[s]. In any event, the final results would *not* differ in any significant deta[il] whether we used the male incidence data, the female incidence dat[a] or the average, which we are using.

The incidence data for carcinoma of the nasal sinuses and mastoi[d] are, as Evans states, not well known, except that these cancers a[re] extremely rare spontaneously. Spratt and coworkers have estimate[d] that cancer of the maxillary antrum is probably less than 1 case pe[r] 200,000 annually (8). These cancers are so rare that the U.S. Vit[al] Statistics do not list them separately. A reasonable estimate woul[d] be that (sinus+mastoid) cancers are ½ as frequent as bone sarcom[a] in the population-at-large, considering the data of Spratt and th[e] U.S. Vital Statistics. We shall use this estimate. This estimate [is] entirely consistent with the estimate by Stewart for England an[d] Wales, prepared for ICRP Publication 11, April 3, 1967.

Item 2: The "Expected" Radiation-Induced Cancers

In the groups of especial interest in this entire analysis (Evan[s] Class a through f—Table 3, Reference 4), Evans presents the follow[-]ing parameters:

Years Since Exposure (average) = 47 years (Figure 5, Refe[r]ence 4)

Average Age in 1967 = 73 years (Figure 5, Reference 4)

Thus, by subtraction, 73 − 47, we deduce the average age where ex[-]posure to radium begins is 26 years.

Further, Evans presents in Table 2 (Reference 4) the followin[g] estimates of how much of the cumulative rad exposure was to [be] expected in the first 10 years of exposure and in the first 40 years [of] exposure. These data are reproduced here (from Table 2, Reference 4[)]:

TYPICAL DOSAGE PARAMETERS FOR 1μCi ^{226}Ra RESIDUAL, 40 YEARS AFTER A BRIEF ACQUISITION (EVAN[S])

	Male (7 Kg.)	Female (5 K[g.])
Cumulative rads in first 10 years	970	1,3[..]
Cumulative rads in first 40 years	2,300	3,?[..]

Inspection of these data shows that most of the cumulative rad do[se] was acquired *after* 10 years. Here we shall make a drastic move to gi[ve] the Evans' "threshold" hypothesis a large advantage. *We shall assum[e] that all the cumulative rads were acquired instantaneously.* In this wa[y] we will *overestimate* the number of cancers *expected* in the years aft[er] exposure. Since the Evans' hypothesis, either of "practical or absolut[e] thresholds of "safe" radiation dose, rests upon a *supposed* discrepan[cy] between "expected" and "observed" cancers, it should be clear th[at] everything we do to increase the "expected" cancers will favor t[he] Evans' hypothesis. As will become evident from the analysis belo[w] even this extreme gesture leaves the Evans' concept unsupportabl[e].

From the Evans' data it appears, as Evans states, that the laten[t] period before cancer occurs is *longer* the lower the cumulative ra[d] delivered. Inspection of Evans' Figure 5 (Reference 4) indicates th[at] for low cumulative rad values latencies in excess of 20 years may occu[r]

We agree that it is reasonable to assume longer latency period at the lower dosages. Therefore, we shall use the following expected latency periods.

Dose Range:	*Average latency period*
0–100 rads	25 years.
100–1200 rads	20 years.
1200–2500 rads	15 years.
>2500 rads	10 years.

One could manipulate such latencies a few years either way, and be perfectly consistent with observational material. The only real issue is to show the *longer* latency period at low doses.

Now, we can proceed with the analysis, based upon cumulative rads, for all the Evans' groups, a through f, and Class 1 through 5 of Table 5 (Reference 4). We shall start with the bone sarcomas, and, after completion of this analysis, go on to the (sinus + mastoid) carcinomas.

Bone Sarcoma: Expected Spontaneous Cancers

Utilizing the data of Table 2, we shall first estimate the *spontaneous* expected bone sarcomas for persons between 26 years of age and 73 years of age. These will be expressed as cases expected spontaneously per 100,000 persons. Later, to estimate *total* expected sarcomas we shall add the calculated radiation-induced cases.

TABLE 3.—EXPECTED SPONTANEOUS BONE SARCOMAS FROM 26 THROUGH 73 YEARS OF AGE

[Number of cases per 100,000 persons]

Age span (in years)	Annual incidence (from table 2)	Number of years in age span	Total incidence for age span
26 to 29	0.30	4	1.20
30 to 34	.22	5	1.10
35 to 39	.27	5	1.35
40 to 44	.32	5	1.60
45 to 49	.68	5	3.40
50 to 54	.92	5	4.60
55 to 59	1.69	5	8.45
60 to 64	2.09	5	10.45
65 to 69	3.06	5	15.30
70 to 73	3.74	4	14.96
Total, age span 26 to 73 years			62.41

Thus, over the entire 47-year span of time, we can expect, from *spontaneous* occurrence, the development of 62.4 sarcomas of bone *per* 100,000 persons at risk.

"Expected" Radiation-Induced Sarcomas of Bone

As stated above, we shall use *strict* linear theory, with 25 rads as the doubling dose for a radiation-induced sarcoma of bone, of a 4 percent increase in risk per rad. The latency periods will be those tabulated above. We shall demonstrate in detail the calculated expectancy of radiation-induced cancers for each dosage group.

The 0–100 Cumulative Rad Dosage Group

For this group we have assumed an average latency period of 25 years. Therefore, for such a group, starting at 26 years of age, the bone sarcomas would start appearing at age 51 years and keep appearing each year thereafter, with 25 rads as doubling dose, to 73 years

of age (where the study group was as of 1967). Therefore, to estimate the radiation-induced contribution, we must first know the spontaneous rate of bone sarcoma occurrence between 51 and 73 years of age. Then we know, from the *definition of doubling dose* that 25 rads produces a number of sarcomas of bone *equal* to this spontaneous rate. For doses above or below 25 rads, we calculate how many doubling doses there are, and then estimate the radiation-induced contribution directly. All of this will be illustrated in detail in the calculations below. The calculated spontaneous expected sarcomas are as follows for the age span 51 years through 73 years (Table 4).

TABLE 4.—EXPECTED SPONTANEOUS CASES (FOR THE PERIOD BEYOND LATENCY)[1]

[Incidence expressed in cases per 100,000 persons]

Age span (years)	Annual incidence (from table 2)	Number of years in age span	Total incidence for age span
51 to 54	0.92	4	3.68
55 to 59	1.69	5	8.45
60 to 64	2.09	5	10.45
65 to 69	3.06	5	15.30
70 to 73	3.74	4	14.96
Total, 51 to 73 years			52.84

[1] Latency: 25 years, 0 to 100 rad-dose group.

So we have 52.84 sarcomas of bone as the spontaneous occurrence in 100,000 persons over the age span of 51–73 years. If 25 rads is the doubling dose, this means, for such a group, every 25 rads adds 52.84 sarcomas of bone per 100,000 persons in this overall age span.

We now proceed serially through all the other dosage groups, with their respective latency periods.

The 100–1200 Cumulative Rad Groups

For this group we have used a latency period of 20 years. Therefore, for such a group, starting at 26 years of age, the bone sarcomas would start appearing at age 46 years, and keep appearing each year thereafter, with 25 rads as the doubling dose, to 73 years of age (where the study group was in 1967). The spontaneous rate of bone sarcoma is needed here too.

TABLE 5.—EXPECTED SPONTANEOUS CASES (FOR THE PERIOD BEYOND LATENCY)[1]

[Incidence expressed in cases per 100,000 persons]

Age span (years)	Annual incidence (from table 2)	Number of years in age span	Total incidence for age span
46 to 49	0.68	4	2.72
50 to 54	.92	5	4.60
55 to 59	1.69	5	8.45
60 to 64	2.09	5	10.45
65 to 69	3.06	5	15.30
70 to 73	3.74	4	14.96
Total, 46 to 73 years			56.48

[1] Latency: 20 years, 100 to 1,200 rad-dose group.

So we have 56.48 sarcomas of bone as the spontaneous occurrence per 100,000 persons over the entire age span 46–73 years.

The 1200–2500 Cumulative Rad Group

For this group we have used a latency period of 15 years. Therefore, for such a group, starting at 26 years of age, the bone sarcomas would start appearing at age 41 years, and keep appearing each year thereafter, with 25 rads as the doubling dose, to 73 years (where the study group was in 1967). The expected spontaneous bone sarcoma incidence for 41–73 years is in Table 6.

TABLE 6.—EXPECTED SPONTANEOUS CASES (FOR THE PERIOD BEYOND LATENCY)[1]

[Incidence expressed in cases per 100,000 persons]

Age span (years)	Annual incidence (from table 2)	Number of years in age span	Total incidence for age span
41 to 44	0.32	4	1.28
45 to 49	.68	5	3.40
50 to 54	.92	5	4.60
55 to 59	1.69	5	8.45
60 to 64	2.09	5	10.45
65 to 69	3.06	5	15.30
70 to 73	3.74	4	14.96
Total, 41 to 73 years			58.44

[1] Latency: 15 years, 1,200 to 2,500 rad-dose group.

So we have 58.44 sarcomas of bone as the expected spontaneous occurrence per 100,000 persons over the age span 41–73 years.

The >2500 Cumulative Rad Group (2500–50,000 cumulative rads)

For this group we have used a latency period of 10 years. Therefore, for such a group, starting at 26 years of age, the bone sarcomas would start appearing at 36 years of age and keep appearing each year thereafter, with 25 rads as the doubling dose, to 73 years (where the study group was in 1967). The expected spontaneous bone sarcoma incidence for 36–73 years is in Table 7.

TABLE 7.—EXPECTED SPONTANEOUS CASES (FOR THE PERIOD BEYOND LATENCY)[1]

[Incidence expressed in cases per 100,000 persons]

Age span (years)	Annual incidence (from table 2)	Number of years in age span	Total incidence for age span
36 to 39	0.27	4	1.08
40 to 44	.32	5	1.60
45 to 49	.68	5	3.40
50 to 54	.92	5	4.60
55 to 59	1.69	5	8.45
60 to 64	2.09	5	10.45
65 to 69	3.06	5	15.30
70 to 73	3.74	4	14.96
Total, 36 to 73 years			59.84

[1] Latency: 10 years, > 2,500 rad-dose group.

So, we have 59.84 sarcomas of bone as the expected spontaneous occurrence per 100,000 persons over the age span 36–73 years.

We can now proceed with the consideration of all the Evans' categories of radium dial painters plus iatrogenic cases.

FORMAL ANALYSIS OF BONE SARCOMA IN THE EVANS' CATEGORIES

Class (a): 170 dial painters plus iatrogenic cases

Dosage range $\geqq 1$ rad to < 50 rads. Median dose 25 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.

Spontaneous bone sarcomas (from 26–73 years) from Table 3, =62.41 per 100,000 persons.

Therefore, for 170 persons, spontaneous expected

$$=\frac{62.41}{100,000}\times 170=0.106 \text{ cases}$$

Radiation-Induced Cases. (Use Table 4, Latency period 25 years)

Spontaneous cases, beyond latency, = 25.84/100,000.

For 25 rads as doubling dose radiation-induced would be=52.84/100,000.

Actual rads for this class = 25 rads. Therefore, 52.84/100,000 = radiation-induced cases.

For 170 persons, radiation-induced cases

$$=\frac{52.84}{100,000}\times 170=0.090 \text{ cases.}$$

Total Bone Sarcomas Expected = spontaneous + radiation-induced = 0.106 + 0.090 = 0.196 cases

Cancer cannot occur as fractional cases. We can observe 0 cases, 1 case, 2 cases, etc.

For expectancy of 0.196 cases, there are 80 chances out of 100 of observing 0 cases. This happened, in accord with probabilities.

Conclusion: Excellent consistency with linear theory out to 50 rads.
No evidence of any safe threshold below 50 rads.
No reason to accept *any* of Evans' claims.

Class (b): 28 radium dial painters plus iatrogenic cases

Dosage range $\geqq 50$ rads to < 100 rads. Median dose 75 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.

Spontaneous bone sarcomas (from 26–73 years) from Table 3, =62.41 per 100,000 persons.

Therefore, for 28 persons, spontaneous bone sarcomas expected

$$=\frac{62.41}{100,000}\times 28=0.017 \text{ cases}$$

Radiation-Induced Cases. (Use Table 4, Latency period 25 years)

Spontaneous cases, beyond latency = 52.84/100,000.

For 25 rads as doubling dose, radiation-induced cases would be 52.84/100,000.

For 75 rads (median dose of this class), radiation-induced

$$=\frac{75}{25}\times 52.84 \text{ per } 100,000=\frac{158.5}{100,000}$$

For 28 persons, radiation-induced cases

$$=\frac{(158.5)(28)}{(100,000)}=0.044 \text{ cases}$$

Total Bone Sarcomas Expected = spontaneous + radiation-induced $=0.017+0.044=0.061$ cases.

There are 94 chances out of 100 of observing 0 cases. This happened.

Conclusion: Excellent consistency with linear theory out to 100 rads.
No evidence for any safe threshold below 100 rads.
No reason to accept *any* of Evans' claims.

Class (c): 41 radium dial painters plus iatrogenic cases

Dosage range $\geqq$ 100 rads to <300 rads. Median dose 200 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.

Spontaneous bone sarcomas (from 26–73 years) from Table 3, $=62.41$ per 100,000 persons.

Therefore, for 41 persons, spontaneous bone sacomas expected

$$=\frac{62.41}{100,000}\times 41=0.026 \text{ cases.}$$

Radiation-Induced Cases. (Use Table 5, Latency period 20 years)
Spontaneous cases, beyond latency $=56.48/100,000$
For 25 rads as doubling dose, radiation-induced cases would be 56.48/100,000
For 200 rads (median dose of this class), radiation-induced

$$=\frac{200}{25}\times\frac{56.48}{100,000}=451.8 \text{ per 100,000 persons}$$

For 41 persons, radiation-induced cases $=\frac{451.8}{100,000}\times 41=0.185$ cases

Total Bone Sarcomas Expected = spontaneous + radiation-induced $=0.026+0.185=0.211$ cases

There are 79 chances out of 100 of observing 0 cases. This happened.

Conclusion: Excellent consistency with linear theory out of 300 rads. No evidence for any safe threshold below 300 rads. No reason to accept *any* of Evan's claims.

Class (d): 17 radium dial painters plus iatrogenic cases

Dosage range $\geqq$300 rads to <600 rads. Median dose 450 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.

Spontaneous bone sarcoma (from 26–73 years) From Table 3, $=62.41$ per 100,000 persons.

Therefore, for 17 persons, spontaneous bone sarcomas expected

$$=\frac{62.41}{100,000}\times 10=0.011 \text{ cases.}$$

Radiation-Induced Cases. (Use Table 5, Latency period 20 years).
Spontaneous cases, beyond latency $=56.48/100,000$.

For 25 rads as doubling dose, radiation-induced cases would be 56.48/100,000.

For 450 rads (median dose of this class), radiation-induced

$$=\frac{450}{25}\times\frac{56.48}{100,000}=1016.6 \text{ cases per 100,000 persons}$$

For 17 persons, radiation-induced cases $=\frac{1016.6}{100,000}\times 17=0.173$ cases.

Total bone sarcomas expected = spotaneous + radiation-induced $=0.011+0.173=0.184$ cases.

There are 82 chances out of 100 of observing 0 cases. This happened.

Conclusion: Excellent consistency with linear theory out of 600 rads. No evidence for any safe threshold below 600 rads. No reason to accept *any* of Evans' claims.

Class (e): 6 radium dial painters plus iatrogenic cases

Dosage range $\geqq$600 rads to <1000 rads. Median dose 800 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.
Spontaneous bone sarcomas (from 26–73 years) from Table 3, $=62.41$ per 100,000 persons.

Therefore, for 6 persons, spontaneous bone sarcomas expected

$$=\frac{62.41}{100,000}\times 6=0.004 \text{ cases}$$

Radiation-Induced Cases (Use Table 5, Latency period of 20 years)
Spontaneous cases, beyond latency $=56.48/100,000$
For 25 rads as doubling dose, radiation-induced cases would be 56.48/100,000
For 800 rads (median dose of this class), radiation-induced

$$=\frac{800}{25}\times\frac{56.48}{100,000}=1807.4 \text{ cases per 100,000 persons.}$$

For 6 persons, radiation-induced cases $=\frac{1807.4}{100,000}\times 6=0.108$ cases.

Total Bone Sarcomas Expected = spontaneous + radiation-induced $=0.004+0.108=0.112$ cases.

There are 89 chances out of 100 of observing 0 cases. This happened.

Conclusion: Excellent consistency with linear theory out to 1000 rads. No evidence of any safe threshold below 1000 rads. No reason to accept *any* of Evans' claims.

Class (f): 5 radium-dial painters plus iatrogenic cases

Dosage Range $\geqq$1000 rads to <1200 rads. Median dose 1100 cumulative rads.

Total bone sarcomas expected = Spontaneous + Radiation-Induced.
Spontaneous bone sarcomas (from 26–73) from Table 3, $=62.41$ per 100,000 persons.

Therefore, for 5 persons, spontaneous bone sarcomas expected

$$=\frac{62.41}{100,000}\times 5=0.003 \text{ cases}$$

Radiation-Induced Cases. (Use Table 5, Latency period 20 years)
Spontaneous cases, beyond latency, $=56.48$ per 100,000 persons
For 25 rads as doubling dose, radiation-induced cases would be 56.48/100,000
For 1100 rads (median dose of this class), radiation-induced

$$=\frac{1100}{25}\times\frac{56.48}{100{,}000}=2485.1 \text{ cases}/100{,}000$$

For 5 persons, radiation-induced cases $\frac{2485.1}{100{,}000}\times 5=0.124$ cases

Total Bone Sarcomas Expected = spontaneous + radiation-induced
$=0.003+0.124=0.127$ cases

There are 87 chances out of 100 of observing 0 cases. This happened.

Conclusion: Excellent consistency with linear theory out to 1200 rads.
No evidence of any safe threshold below 1200 rads.
No reason to accept *any* of Evans' claims.

Combined Analysis of All Classes (a) through (f)

There is an easy way to determine whether taking all classes together will in any way help the Evans' hypothesis.

(a) First, we can proceed to calculate the probability of observing 0 cases for all the groups combined. Since each group is independent of the others, the probability of observing 0 cases in *all* the groups is the simple product of the individual probabilities.

Thus, (a) × (b) × (c) × (d) × (e) × (f)

$$\text{Combined Probability}=\frac{80}{100}\times\frac{94}{100}\times\frac{79}{100}\times\frac{82}{100}\times\frac{89}{100}\times\frac{87}{100}$$

$=38$ chances out of 100 of observing 0 cases. This happened.

Obviously, this is *well* within the realm of chance. Indeed, if anyone draws any comfort out of the fact that 38/100 is below 50/100, he simply doesn't understand statistical probabilities. Even more, it so happens that sitting on the upper edge of the combined (a) through (f) group is a case of bone sarcoma at 1200 cumulative rads. No one even dreams that the assignment of dose is good to 50 rads out of 1200. Thus, had we used 1201 rads as the cutting line instead of 1200 rads, we would have

Combined probability of observing 0 cases = 38/100
probability of observing 1 case ≅ 62/100
One case was observed.

So, we can again state:

Conclusion: Excellent consistency with linear theory from 0–1200 rads.
No evidence for any safe threshold from 0 to 1200 rads.
No reason to accept *any* of Evans' claims.

The High Dose Region—Test of the Linear Theory

In refuting the *purported* evidence for a safe "threshold" of radiation all the way out to 1200 rads of cumulative radiation, we have estimated the expected spontaneous + radiation-induced cases based upon linear theory. We shall now add, group by group, the extremely high dose groups where a large number of cancers were observed.

We shall thereby have an opportunity to assess the linear theory into the very high dose range.

In table 5 of Reference 4, Evans presents the data for his Class 1–5 cases, designated as statistically suitable cases with a very high incidence of bone sarcoma plus (sinus + mastoid) carcinoma. We shall accept this group for analysis, even though we have many epidemiological reservations about *all* of the Evans' cases. (see Discussion above) Provided such reservations are kept in mind, the analysis can proceed. Reproduced here are Evans' data for Class 1–5 subjects. (Table 8)

TABLE 8

Dose range [1]	Median dose [1]	Number of subjects	Number of bone sarcomas	Number of sinus plus mastoid carcinomas
1,200 to 2,500	1,850	12	4	0
2,500 to 5,000	3,750	22	3	2
5,000 to 10,000	7,500	12	2	1
10,000 to 20,000	15,000	8	1	2
20,000 to 50,000	35,000	5	0	2
Total		59	10	7

[1] Cumulative rads.

In this part of our analysis, we are concerned only with the bone sarcomas. We shall return to the separate problem of the (mastoid + sinus) carcinomas later.

Class 1: Calculated Contribution of Spontaneous + Radiation-Induced Bone Sarcomas

12 radium dial painters plus iatrogenic cases.
Dosage range ≥1200 to <2500 rads. Median dose 1850 rads.
Total bone sarcomas = Spontaneous + Radiation-Induced.
Spontaneous bone sarcomas (from 26–73 years) from Table 3 = 62.41 per 100,000 persons.

Therefore, for 12 persons, spontaneous bone sarcoma

$$=\frac{62.41}{100{,}000}\times 12=\underline{\underline{0.017 \text{ cases.}}}$$

Radiation-Induced Cases (Use Table 6, Latency period 15 years)
Spontaneous cases, beyond latency, $=58.44$ cases per 100,000 persons.
For 25 rads as doubling dose, the radiation-induced cases would be $=58.44/100{,}000$ persons

For 1850 rads (median dose for this class), radiation-induced is

$$\frac{1850}{25}\times\frac{58.44}{100{,}000}=4324.6 \text{ per } 100{,}000.$$

Therefore, for 12 persons, radiation-induced bone sarcomas

$$=\frac{4324.6}{100{,}000}\times 12=\underline{\underline{0.52 \text{ cases.}}}$$

Total bone sarcomas expected = Spontaneous + Radiation-induced

$=0.003+0.52=0.523$ cases

Now we can total up all the expected from 0 rads out to 2500 rads.

Expected Class a	=	0.096
" b	=	0.061
" c	=	0.211
" d	=	0.184
" e	=	0.112
" f	=	0.127
Class 1	=	0.523

Total Expected out of 2500 rads 1.414 Observed 4 cases.

In the statistics of small numbers, this difference is not provably significant, and its spurious nature becomes evident as we bring in the remaining data in the high dose ranges.

Class 2: 22 radium dial painters iatrogenic cases

Dosage range ≥ 2500 to < 5000 rads. Median dose 3750 cumulative rads.

Total bone sarcomas = Spontaneous + Radiation-Induced.

Spontaneous, as above, = 62.41 per 100,000 persons.

Therefore, for 22 persons, spontaneous $= \frac{62.41}{100,000} \times 22 = 0.014$ cases.

Radiation-Induced Cases (Use Table 7, Latency period 10 years)
Spantaneous cases, beyond latency = 59.84/100,000 persons.
For 25 rads as doubling dose, the radiation-induced cases would be = 59.84/100,000 persons.

Therefore, for 3750 rads (median dose for this class) radiation induced is

$$\frac{3750}{25} \times \frac{59.84}{100,000} = 8976 \text{ per } 100,000.$$

For 22 persons, radiation-induced $\frac{8976}{100,000} \times 22 = 1.97$ cases.

Total cases = Spontaneous + Radiation-Induced
= 0.014 + 1.97 = 1.984 cases

Now, we can again total all the expected—this time from 0 out to 5000 rads.

Expected, Class a through f + Class 1	=	1.414
Class 2	=	1.984
		3.398, or 3.40 cases expected

Observed = 4 + 3 = 7 cases total.

The observed is still slightly higher than predicted, but not significantly so. Of much greater relevance is completion of the analysis to include the remaining cases.

Class 3: 12 radium dial painters iatrogenic cases

Dosage range ≥ 5000 to < 10000 rads. Median dose 7500 cumulative rads.

Total bone sarcomas = Spontaneous + Radiation-Induced.

Spontaneous, as above, $= \frac{62.41}{100,000} \times 12 = 0.007$ cases.

Radiation-Induced Cases (Use Table 7, Latency period 10 years)
Spontaneous cases, beyond latency, = 59.84/100,000 persons
For 25 rads as doubling dose, the radiation-induced cases would be = 59.84/100,000 persons.
Therefore, for 7500 rads (median dose for this class) radiation-induced is $\frac{7500}{25} \times \frac{59.84}{100,000} = 17952$ per 100,000

So, for 12 persons, radiation induced cases $= \frac{17952}{100,000} \times 12 = 2.15$ cases

Total cases = Spontaneous + Radiation-Induced = 0.007 + 2.15 = 2.157 cases

Now, we can *total* all the expected—this time from 0 rads out to 10,000 rads.

Class a–f + Class 1 + Class 2 = 3.40 cases expected
Class 3 = 2.16 cases expected

All Classes out to 10,000 rads = 5.56 cases expected
All Classes out to 10,000 rads = 4 + 3 + 2 = 9 cases observed.

Again, the difference is not significant, and the values are coming closer together.

Class 4: 8 radium dial painters + iatrogenic cases

Dosage range ≥ 1000 to < 20000 rads. Median dose 15000 cumulative rads.

Total bone sarcomas = Spontaneous + Radiation-Induced.

Spontaneous, as above, $= \frac{62.41}{100,000} \times 8 = 0.005$ cases.

Radiation-Induced Cases (Use Table 7, Latency period 10 years)
Spontaneous cases, beyond latency = 59.84/100,000 persons.
For 25 rads as doubling dose, the radiation-induced would be = 59.84/100,000 persons.
For 15000 rads (median dose for this class), radiation-induced is

$\frac{15000}{25} \times \frac{59.84}{100,000} = 35{,}904$ per 100,000 persons

So, for 8 persons, radiation-induced bone sarcomas =

$\frac{35,904}{100,000} \times 8 = 2.87$ cases.

Total cases expected = Spontaneous + Radiation-Induced = 0.005 + 2.87 = 2.88 cases

Now we can total all the expected—this time from 0 rads out to 20,000 rads.

Class a–f + Class 1 + Class 2 + Class 3 = 5.56
Class 4 = 2.88

All Classes out to 20,000 rads = 8.44 cases expected
All Classes out to 20,000 rads = 4 + 3 + 2 + 1 = 10 cases observed.

These expected and observed values are nearly identical.

Class 5: 5 cases of radium dial painters + iatrogenic cases

Dosage range $>20{,}000$ to $<50{,}000$ rads. Median dose 35,000 cumulative rads.

Total bone sarcomas = Spontaneous + Radiation-Induced.

Spontaneous, as above, $\frac{62.41}{100{,}000} \times 5 = 0.003$ cases.

Radiation-Induced Cases (Use Table 7, Latency period 10 years)
Spontaneous cases, beyond latency $= 59.84/100{,}000$ persons
For 25 rads as doubling dose, the radiation-induced would be $= 59.84/100{,}000$ persons.
For 35,000 rads (median dose for this class) radiation induced is

$$\frac{35{,}000}{25} \times \frac{59.84}{100{,}000} = 83{,}776 \text{ per } 100{,}000.$$

So, for 5 persons, radiation-induced cancer $\frac{83776}{100{,}000} \times 5 = 4.19$ cases.

Total cases = Spontaneous + Radiation-Induced $= 0.003 + 4.19 = 4.193$ cases

Now we are in a position to obtain *Grand Totals*, including all the High Dose Domain plus the Low Dose Domain.

Expected, Class a-f + Class 1 + Class 2 + Class 3 +

Class 4 =	8.43 cases
Class 5 =	4.19 cases
(0 to 50,000 rads) *Grand Total, Expected*	= 12.62 cases
(0 to 50,000 rads) *Grand Total, Observed*	= 10 cases

Realizing the statistical error in this small number of observed cases, this agreement is nothing short of fantastic!

Simple linear theory, using a doubling dose of 25 rads (for α radiation), over a tremendous range, 0–50,000 rads, has accomplished the following:

(a) Demonstrated that no evidence whatever for a safe threshold exists anywhere from 0 to 50,000 rads.

(b) Given excellent agreement in predicting the dose response curve giving agreement with observed bone sarcomas in high dose domain.

(c) Demonstrated that no reason whatever exists to accept *any* of Evans' hypotheses.

We are indeed surprised that linear theory can explain the observations over a dose domain 10,000-fold higher than that relevant for setting FRC Guidelines. While this analysis clearly shows Evans cannot possibly demonstrate a threshold, it is possible that a slight curvature in the high part of the dose-effect curve cannot be ruled out. For example, possibly 30 rads might be the doubling does out to 1000–1500 rads followed by a gentle curvature to 20 rads as doubling dose. We suspect not even the staunch advocates of strict linear theory would consider this much of a concession. We certainly concede this possibility.

At the same time, this analysis demonstrates again the wide usefulness of the doubling dose concept in human radiation carcinogenesis.

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FORMAL ANALYSIS OF (SINUS PLUS MASTOID) CARCINOMAS IN THE EVANS' CATEGORIES

We can now proceed to the analysis of the sinus + mastoid carcinomas, studying the radium dial painters plus the iatrogenic cases.

As a best estimate for the spontaneous incidence of (sinus + mastoid) cancers, we shall everywhere use ½ the incidence of bone sarcomas. This was explained above. Since many of the calculations are identical with those for bone sarcoma, all that will be needed is the factor of ½ to correct for spontaneous incidence.

In accordance with our general laws for radiation carcinogenesis in humans, we shall, of course, retain the doubling dose of 25 rads (for α radiation) for (sinus + mastoid) carcinoma induction.

Class (a): The 170 radium dial painters plus iatrogenic cases

Dosage range ≥ 1 to <50 rads. Median dose 25 cumulative rads.
Spontaneous expected (sinus + mastoid) Cancers = ½ Bone Sarcomas Expected $= \frac{1}{2} \times 0.106 = 0.053$ cases
Radiation-Induced Cases (Table 4, Latency period 25 years)
The estimate for bone sarcomas, radiation-induced was 0.090 case
Therefore, radiation-induced (sinus + mastoid) carcinomas $= \frac{1}{2} \times 0.090 = 0.045$
(Sinus + Mastoid) Total = Spontaneous + Radiation-Induced Carcinomas $= 0.053 + 0.045 = 0.098$ Expected Cases
There are 90 chances out of 100 of observing 0 cases. This happened.

Class (b): 28 radium dial painters + iatrogenic cases

Dosage range ≥ 50 to <100 rads. Median dose 75 cumulative rads.
As was noted for class (a), the nature of the calculation is such that the final expected for (sinus + mastoid) cancers = ½ that calculated for bone sarcomas.
So, from previously, Total Expected Bone Sarcomas = 0.061 cases
Therefore, total expected (Sinus + Mastoid) Cancers $= \frac{1}{2} \times 0.061 = 0.030$ cases
There are 97 chances out of 100 of observing 0 Cases. This happened.

Class (c): 41 radium dial painters plus iatrogenic cases

Dosage range ≥ 100 to < 300 rads. Median dose 200 cumulative rads.
From previously, total expected bone sarcomas = 0.211 cases.
Therefore, total expected (sinus + mastoid) cancers $= \frac{1}{2} \times 0.211 = 0.105$ cases.
There are 89 chances out of 100 of observing 0 cases. This happened.

Class (d): 17 radium dial painters plus iatrogenic cases

Dosage range ≥ 300 to < 600 rads. Median dose 450 cumulative rads.
From previously, total expected bone sarcomas = 0.184 cases.
Therefore, total expected (sinus + mastoid) cancers $= \frac{1}{2} \times 0.184 = 0.092$ cases.

There are 91 chances out of 100 of observing 0 cases. This happened.

Class (e): 6 radium dial painters plus iatrogenic cases

Dosage range ≥ 600 to < 1000 rads. Median dose 800 cumulative rads.

From previously, total expected bone sarcomas = 0.112 cases.

Therefore, total expected (sinus + mastoid) cancers $= \frac{1}{2} \times 0.184 = 0.056$ cases.

There are 94 chances out of 100 of observing 0 cases. This happened.

Class (f): 5 radium dial painters plus iatrogenic cases

Dosage range ≥ 1000 rads to < 1200 rads. Median dose 1100 cumulative rads.

From previously, total expected bone sarcomas = 0.127 cases.

Therefore, total expected (sinus + mastoid) cancers $= \frac{1}{2} \times 0.127 = 0.063$ cases.

The findings on each class a through f, for (sinus + mastoid) cancer, are like they were for bone sarcoma, leading to:

Conclusion: Excellent consistency with linear theory out to 1200 rads.
No evidence whatever for a safe threshold out to 1200 rads.
No reason to accept *any* of Evans' claims.

Combined Analysis for (Sinus + Mastoid) Cancer for Classes (a) through (f)

Proceeding just as we did for bone sarcoma, we can now estimate the combined probability of observing 0 cases in all the classes combined.

$$\text{Probability} = (a)\times(b)\times(c)\times(d)\times(e)\times(f)$$
$$= \frac{90}{100}\times\frac{97}{100}\times\frac{89}{100}\times\frac{91}{100}\times\frac{94}{100}\times\frac{94}{100}$$

=62 chances out of 100 is probability of observing 0 cases

0 cases were observed.

Conclusion: Excellent consistency with linear theory with combined groups.
No evidence for any safe threshold with combined groups.
No reason whatever to accept *any* Evans' claims.

The High Dose Region: Tests of Linear Theory Using (Sinus + Mastoid) Cancer

Class 1: 12 radium dial painters + iatrogenic cases

Dosage range ≥ 1200 to < 2500 rads. Median dose 1850 rads.

Total bone sarcomas expected = 0.523 cases.

Therefore, (total sinus + mastoid) cancers $= \frac{1}{2} \times 0.523 = 0.261$ cases.

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Now we can total expected (sinus + mastoid) cancers from 0 through 2500 rads:

Class	a = 0.098	cases
	b = 0.030	"
	c = 0.105	"
	d = 0.092	"
	e = 0.056	"
	f = 0.063	"
Class	1 = 0.261	"

Total Expected (sinus + mastoid) 0.705 cases
Observed to 0–2500 rads = 0 cases

The expected and observed are not significantly different this far into the high dose range.

Class 2: 22 radium dial painters + iatrogenic cases

Dosage range ≥2500 to <5000 rads. Mean dose 3750 cumulative rads.

Total bone sarcomas expected = 1.984 cases.

Therefore, total expected (sinus + mastoid) cancers = 0.992 cases.

Now we can again total all the expected—this time out to 5000 rads.

Class a − f + Class 1 + 2 = 1.697 *expected* cases (sinus + mastoid) cancers
" + " = 0 + 2 = 2 *Observed* (sinus + mastoid) cancers

These are not provably different—out to 5000 rads.

Class 3: 12 radium dial painters + iatrogenic cases

Dosage range ≥5000 to <10000 rads. Median dose—7500 cumulative rads.

Total expected bone sarcoma = 2.157 cases.

Therefore, total expected (sinus + mastoid) cancers = 1.078 cases.

Now we can total all expected cases—out to 10000 rads.

Class a–f + Class 1 + Class 2 = 1.697 cases
Class 3 = 1.078 cases

All Classes out to 10,000 rads = 2.775 cases *expected* (sinus + mastoid) cancers

Class a–f + Class 1 + 2 + 3 = 2 + 1 = 3 cases *observed.*

These are not provably different—out of 10,000 rads.

Class 4: 8 radium dial painters + iatrogenic cases

Dosage range ≥10000 to <20000 rads. Median dose 15000 cumulative rads.

Total bone sarcomas expected = 2.88 cases.

Therefore, total expected (sinus + mastoid) cancers = 1.44 cases

Now we can total all expected cases—out to 20000 rads.

Class a–f
+ Class 1 + 2 + 3 = 2.775 cases
Class 4 = 1.44 cases

Class a–f + Class 1–4 = 4.215 *expected* cases (sinus + mastoid) cancers

These are not provably different—out to 20000 rads.

Class 5: 5 cases of radium dial painters + iatrogenic cases

Dosage range > 20000 to < 50000 rads. Median dose 3500 cumulative rads.

Total expected bone sarcomas = 4.19 cases

Therefore, expected (sinus + mastoid) cancers = 2.09 cases.

Now, we are in a position to obtain *Grand Totals*, including all the High Dose Domain.

Expected (Sinus + Mastoid) Cancers,

Class a–f + Class 1–4 = 4.215 cases

Class 5 = 2.09 cases

Grand Total, expected (sinus + mastoid) cancers = 6.305 cases

Grand Total, observed = 7 cases.

We can regard this as fabulously good agreement.

Thus, just as for the bone sarcomas, complete analysis of the (sinus + mastoid) cancer data shows:

(a) Complete consistency with linear theory from 0–50000 rads in predicting total number of cases.

(b) Excellent consistency in the lower dose domain (below 1200 rads) and demonstrating that there is *no* evidence supporting a threshold.

(c) Linear theory accords with data, using a single value for doubling dose, 25 rads, from 0 cumulative rads to 50,000 cumulative rads.

THE THOROTRAST CASES OF MARINELLI

Evans draws comfort from the observations of Marinelli on some 2000 persons who have carried Thorotrast deposits for 20–25 years[9]. He estimates their burdens of skeletal α-emitters to correspond to cumulative rad values between 30 and 80 rads. Let us quote Evans directly:

"These burdens would correspond to a cumulative rad value of about 30–80 rads, and on the linear non-threshold hypothesis illustrated by curve 3 in Figure 10 (Reference 4), should be responsible for some 15 to 40 cases of sarcoma compared with the observed value of 1 or possible 2."

We shall accept Dr. Evans' calculation of cumulative rad burdens. However, we haven't the vaguest notion why he picked a "strawman" curve such as his curve 3, Figure 10 (Reference 4). That linear curve is *much* too steep. We certainly agree with Evans that his curve 3 gives too high a value.

Let us go through the Thorotrast data with our simple linear theory using the 25 rad doubling dose, which has proved to agree so beautifully with Evans' *data*. As a first approximation, we shall assume all the ages, etc. were as in the Evans' series, although a refined treatment can readily be performed.

For most of our data, a rounded spontaneous value, beyond latency, is ~55 bone sarcomas per 100,000 persons.

For 25 rads doubling dose, this means 55 bone sarcomas per 100,000

Let us use Evans' calculated rad limits 30–80 rads.

For 30 rads, radiation-induced sarcomas

$$=\frac{30}{25}\times 55/100{,}000=66/100{,}000$$

For 2000 persons, expected radiation-induced sarcomas

$$=\frac{66}{100000}\times 2000=1.32 \text{ cases.}$$

For 80 rads, radiation-induced would be $\frac{80}{30}\times 1.32=3.5$ cases.

Now, the total follow-up period in the Thorotrast series is *less* than the radium series, so we know that the estimates above are too high. So we calculate, as a first approximation:

For 30 rads < 1.32 bone sarcomas expected

For 80 rads < 3.5 bone sarcomas expected.

This is clearly in accord with Evans' statement that 1 or possibly 2 cases were observed. *Our* linear model is in no disagreement whatever with the Thorotrast data.

THE CHEMISTS EXPOSED TO RADIUM

The radium dial painters and iatrogenic cases we have analyzed above. Evans presents an additional 139 cases labelled "Chemists and Miscellaneous" who received exposures in the domain 0–1200 cumulative rads. The cumulative rad dosages, reproduced from Table 3, Reference 4, are as follows:

"CHEMISTS AND MISCELLANEOUS"—ALL DOSES IN CUMULATIVE RADS

Class	Number of persons	Dosage range	Median dose
a	106	≥1 to <50	25
b	6	≥50 to <100	75
c	20	≥100 to <300	200
d	0	≥300 to <600	450
e	7	≥600 to <1,000	800
f	0	≥1,000 to 1,200	1,100
Total	139		

No cancers were observed in any of these cases. Let us now ascertain whether this suggests any threshold or non-linear response.

We shall assume,

(a) Linear response over entire domain.

(b) 25 rads as the doubling dose for bone sarcoma, as before.

As Evans points out, the chemists were older, on the average, when they were exposed and, further, they received their dose over an extended period of time.

Thus, to be rigorous, we should credit this group with a longer effective latent period than the dial painters and, hence, a shorter average period to be developing bone sarcoma. If, therefore, we lean over backwards to favor the Evans' hypothesis, we can assume that *all* the cumulative rads were acquired instantaneously, and that the *same* number of years were available for sarcoma development as in the dial painters plus iatrogenic cases. These approximations tend to *raise* the *expected* cancers and, hence, favor Evans in a domain where 0 cancers were observed.

Let us now calculate the expected numbers of cancers for this group of 139 total cases.

Spontaneous bone sarcomas expected

Spontaneous bone sarcomas = 62.41/100,000

So far 139 cases, spontaneous bone sarcomas $= \frac{62.41}{100000} \times 139 = 0.087$ cases

Radiation-induced bone sarcomas

Average dose, in cumulative rads, for entire group

$$= \frac{(106)(25) + (6)(75) + (20)(200) + (7)(800)}{139}$$

$$= \frac{2650 + 450 + 4000 + 5600}{139} = 91.4 \text{ cumulative rads.}$$

Use the data of Table 4 (which is for 0–100 cumulative rads) 52.84 cases per 100,000 is the spontaneous bone sarcoma occurrence, beyond the latency.

For 25 rads as doubling dose, then 25 rads will produce 52.84 radiation-induced cases per 100,000 persons.

For 91.4 cumulative rads,

Radiation-induced bone sarcomas is $\frac{91.4}{25} \times 52.84/100{,}000$

$= 193.2$ per 100,000

Therefore, for 139 persons, radiation-induced bone sarcomas

$= \frac{193.2}{100000} \times 139 = 0.269$ cases.

Total Expected = Spontaneous + Radiation-Induced
$= 0.087 + 0.269 = 0.346$ cases.

There are 65 chances out of 100 of observing 0 cases. This happened. Note: Had we treated this group more rigorously, instead of favoring Evans' hypothesis on several points, the calculated chance of observing 0 cases would have been between 80–90 out of 100. But, 65 out of 100 is good enough to say:

(a) Excellent consistency with the linear theory of radiation-carcinogenesis in man.

(b) No evidence whatever for any safe threshold out to 1200 rads.

(c) No reason whatever to accept *any* of Evans' claims. This exhausts the various radium-exposed persons to be analyzed.

CUMULATIVE RADS AND CUMULATIVE RAD-YEARS

Evans bases his analyses upon cumulative rads, but suggests cumulative rad-years is even better. It certainly should be! Thus, if we had a series of persons who have received 2000 cumulative rads to the skeleton and we look at them one year later (long before the latency period is over), we would find essentially no cancers. This is no surprise.

Our analysis takes both cumulative rads *and* cumulative rad-years into account, for it *credits* the cumulative rads with a risk of cancer induction for *each year of life beyond the latency period.*

CONCLUSION

In his paper (Reference 4), Evans admonishes the adherents of the linear, non-threshold hypothesis as follows: (Direct quote)

"Those compilers who publish risk-estimates based upon a linear non-threshold model and their reading of the MIT and ANL–ACRH Ra and MsTh data are perhaps unaware of the mathematical odds against their proposed numbers."

We shall accept the appellation of being two such "compilers" and, further, shall compliment Dr. Evans sincerely for the massive and excellent data he has provided.

And we say to Dr. Evans,

"No, Dr. Evans, the mathematical odds are *not at all* against us. At every step in the analysis we find the mathematical odds completely consistent with our predictions versus observation. We suggest, furthermore, that if you, Dr. Evans, try a *realistic* linear model such as ours, you'll be most pleased with the agreement with the data—out to 50,000 rads".

Lastly, we must quote from our previous report where we demonstrated, using Residual Ra Burden, the lack of validity of Evans' insistence that "the linear non-threshold model is incorrect".

"If it is true that NCRP, ICRP and AEC, as Evans suggests, used these studies to decide permissible burdens of radium, plutonium, and strontium-90, they would be well advised to cease and desist from any such further use". (1)

This statement is even more relevant now that we have analyzed Evans' recent claims.

ACKNOWLEDGMENT

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The Colorado Plateau: Joachimsthal Revisited?

An Analysis of the Lung Cancer Problem in Uranium and Hardrock Miners

(By Arthur R. Tamplin and John W. Gofman)

[This is Document No. 4 in a Series of 7. Previously issued: No. 3]

INTRODUCTION

There is no longer any significant debate concerning whether or not the inhalation of radon daughters in uranium and hardrock mining is associated with an excessively high incidence of lung cancer in the miners (1) (2). Among the items at issue are:

(*a*) Type of cancer induced.
(*b*) Shape of the dose-effect curves.
(*c*) Correction for effect of cigarette smoking.
(*d*) Presence or absence of a "threshold."
(*e*) Appropriate standards for uranium and hardrock mining.

An excellent set of hearings on the overall subject was conducted by the Joint Committee on Atomic Energy (3). In spite of the quality of the presentations there, it is interesting that an enormous amount of confusion has been shed upon this subject. That the Joachimsthal tragedy has, in part at least, been repeated on the Colorado Plateau is regrettable, but it is past. What will indeed be difficult to understand will be a revisit to Joachimsthal in the future.

Dr. Carl Walske, of the Federal Radiation Council, recently requested us to re-study the findings in the JCAE Hearings concerning the uranium miners. It is the purpose of this report to present our analysis of this problem. We propose to address all the issues noted above.

I. THE INPUT DATA CONCERNING LUNG CANCER IN URANIUM AND HARDROCK MINERS

In FRC Report No. 8 (Preliminary Staff Report, May, 1967) are presented the potential input data for 49 cases of lung cancer among 1981 uranium miners who started mining before 1955 (4). Some debate occurred in the Epidemiologic-Statistical Subgroup as to which cases should or should not be included in a proper assessment of the problem. (5) Several important epidemiologic issues were properly and beautifully discussed by this group concerning case exclusion. Much of this centers around obtaining an appropriate "base" population estimate of *expected* lung cancers *without* exposure to radon daughters. This is excellent epidemiological thinking. However, the primary purpose of ascertainment that radon daughter exposure is related to excessive lung cancers is no longer an issue. Therefore, we must use the data for the remaining aspects of the problem listed in

(*a*) through (*e*) above. For these purposes, we are not nearly so concerned about the *precise* estimates of expected lung cancers in non-exposed persons. Rather we are concerned about such issues as dose vs. effect, "thresholds", and types of cancer that are radiation-induced. As we shall show below, no material alteration in the substance of the conclusions would occur even if the "expected" incidence in non-exposed persons were *twice* that presented by the Epidemiologic Statistical Subgroup. Since this is true, we shall be able to use a larger fraction of the data pool than is in Table 4 of the Epidemiologic-Statistical Subgroup (5).

Next, this Subgroup indicated that the use of Death Certificate diagnosis did not materially alter the distribution of cases when contrasted with the results of careful clinico-pathologic study of autopsy or biopsy material. However, again because Death Certificate ascertainment was the basis for calculating expected incidence in non-exposed persons, they preferred to keep this requirement for inclusion of exposed cases. Again, because we shall show that even a doubling of "expected" incidence in non-exposed persons doesn't alter the picture, we shall utilize a larger fraction of the cases—namely, those from death certification *or* clinico-pathologic study. However, we shall eliminate those cases where the careful clinico-pathologic study indicated the cancer *not* to be lung cancer, primary.

Next, the careful re-study of mining exposure led to the realization that some of the radon-daughter exposure was experienced in hardrock mining other than uranium mining. As a result, the low exposure categories tended to move up slightly when this additional exposure was taken into account. However, since similar corrections were not made for the base population, the Epidemiologic-Statistical Subgroup preferred to retain the case distribution based upon uranium mining exposure alone. We shall do this, but we shall *also* demonstrate that correction for the exposure due to hardrock mining would not materially alter *any* conclusions concerning dose-effect relationships, "thresholds", type of cancer induced by radiation, or appropriate guidelines for mine exposure to radon daughters.

Further, since it is well-known that a latency period exists before radiation-induced cancer becomes manifest, we too shall exclude cases of lung cancer occurring *less than* 5 years after the beginning of exposure.

TABLE 1.—49 CASES OF LUNG CANCER FOR POTENTIAL INCLUSION IN STUDY OCCURRING IN 1,981 MINERS WHO BEGAN MINING BEFORE JULY 1955

[Reproduced from app. A of FRC Rept. 8 (preliminary). See reference 4]

FRC case No.	Exposure category, WLM (uranium exposure only) (see note 4)	Date of death	Beginning of mining	Exclusion basis, if excluded
1	<120 WLM (A)	Mar. 16, 1962	1951	Included.
2	<120 WLM (A)	Dec. 6, 1965	1950	Do.
3	<120 WLM (A)	Jan. 4, 1965	1954	Do.
4	<120 WLM (A)	July 24, 1958	1955	Excluded, death only 3 years later.
5	120-359 WLM (B)	Oct. 21, 1965	1952	Included.
6	120-359 WLM (B)	Sept 20, 1962	1949	Do.
7	120-359 WLM (B)	Mar. 1, 1964	1948	Excluded, not primary lung cancer.
8	120-359 WLM (B)	Feb. 22, 1965	1949	Included.
9	120-359 WLM (B)	Dec. 22, 1962	1952	Do.
10	120-359 WLM (B)	May 2, 1964	1953	Do.
11	120-359 WLM (B)	....	1953	Error in FRC (8) which shows him dying 4 years before he quit mining. Included.

TABLE 1.—49 CASES OF LUNG CANCER FOR POTENTIAL INCLUSION IN STUDY OCCURRING IN 1,981 MINERS WHO BEGAN MINING BEFORE JULY 1955—Continued

FRC case No.	Exposure category, WLM (uranium exposure only) (see note 4)	Date of death	Beginning of mining	Exclusion basis, if excluded
12	360-839 WLM (C)	June 19, 1964	1954	Included.
13	360-839 WLM (C)	June 28, 1963	1951	Do.
14	360-839 WLM (C)	Nov. 25, 1963	1950	Do.
15	360-839 WLM (C)	July 5, 1965	1952	Do.
16	360-839 WLM (C)	Dec. 5, 1965	1952	Do.
17	840-1799 WLM (D)	Apr. 12, 1964	1939	Do.
18	840-1799 WLM (D)	May 18, 1964	1945	Do.
19	840-1799 WLM (D)	Sept. 15, 1963	1940	Do.
20	840-1799 WLM (D)	Mar. 5, 1965	1940	Do.
21	840-1799 WLM (D)	Dec. 9, 1965	1947	Do.
22	840-1799 WLM (D)	May 29, 1965	1955	Do.
23	840-1799 WLM (D)	Oct. 31, 1961	1954	Do.
24	840-1799 WLM (D)	Aug. 23, 1963	1952	Do.
25	840-1799 WLM (D)	Dec. 29, 1969	1951	Do.
26	840-1799 WLM (D)	Jan. 13, 1966	1952	Do.
27	840-1799 WLM (D)	June 15, 1969	1953	Do.
28	1800-3719 WLM (E)	Feb. 14, 1956	1940	Do.
29	1800-3719 WLM (E)	Nov. 19, 1956	1940	Do.
30	1800-3719 WLM (E)	Oct. 20, 1962	1939	Do.
31	1800-3719 WLM (E)	May 4, 1963	1937	Do.
32	1800-3719 WLM (E)	Aug. 15, 1964	1940	Do.
33	1800-3719 WLM (E)	Dec. 23, 1963	1941	Do.
34	1800-3719 WLM (E)	Feb. 5, 1961	1944	Do.
35	1800-3719 WLM (E)	Mar. 23, 1963	1940	Do.
36	1800-3719 WLM (E)	June 29, 1965	1948	Do.
37	1800-3719 WLM (E)	Jan. 14, 1961	1949	Excluded, not primary lung cancer.
38	>3720 WLM (F)	Nov. 3, 1955	1925	Included.
39	>3720 WLM (F)	Feb. 14, 1957	1941	Do.
40	>3720 WLM (F)	Aug. 29, 1957	1945	Excluded, not primary lung cancer.
41	>3720 WLM (F)	May 22, 1965	1939	Included.
42	>3720 WLM (F)	Nov. 15, 1958	1949	Do.
43	>3720 WLM (F)	Dec. 25, 1960	1944	Do.
44	>3720 WLM (F)	Dec. 29, 1965	1940	Do.
45	>3720 WLM (F)	Nov. 19, 1963	1954	Do.
46	>3720 WLM (F)	May 4, 1959	1951	Do.
47	>3720 WLM (F)	Apr. 10, 1964	1950	Do.
48	>3720 WLM (F)	Feb. 20, 1966	1950	Do.
49	>3720 WLM (F)	Aug. 26, 1965	1950	Do.

NOTES

1. Wherever clinico-pathology corrected death certificate diagnosis, the former was used.
2. Cases are included, where either death certificate or clinico-pathology indicated primary lung cancer, subject to proviso (1).
3. Cases 2, 5, 15, 16, 21, 26, 44, 48, 49 died soon after the June 1965 cutoff date. (Average few months.) The correction of "expected" lung cancers for inclusion of these cases, purposely made generous, is described in text. It would appear impossible that any dose-category bias is thereby introduced.
4. Correction for hardrock mining radon-daughter exposure is described in text.

TABLE 2.—45 CASES OF LUNG CANCER IN RADON-DAUGHTER EXPOSED URANIUM MINERS BY WLM EXPOSURE CATEGORY [1]

Exposure category	Estimated cumulative (WLM) range	Estimated cumulative (WLM) median	Number of Miners in base population	Number of cancers
A	<120	<60	383	3
B	120-359	240	421	6
C	360-839	600	496	5
D	840-1,799	1,320	400	11
E	1,800-3,719	2,760	218	9
F	≥3,720	~6,000	63	11
Totals			1,981	45

[1] Bases for inclusion thoroughly discussed in text plus footnotes to table 1.

Lastly, there are several cases who died of lung cancer shortly after the cut-off date (June 1965) for the original formal analysis. The Epidemiologic-Statistical Subgroup excluded such cases because they wanted to check for complete ascertainment in this post-June 1965

period, as well as to check that the Death Certification was precisely like the other cases. However, for the purposes of this analysis, it is perfectly acceptable to include such cases, and increase the "expected" cancers generously to account for them.

In Table 1 are listed the 49 lung cancer cases available for potential inclusion. We shall exclude the following cases and provide the reason for exclusion.

Case 4: Excluded because death occurred 3 years after beginning of mining.

Case 7: Clinico-pathology diagnosis "*Not* primary lung cancer".

Case 37: Clinico-pathology diagnosis "*Not* primary lung cancer".

Case 40: Clinico-pathology diagnosis "*Not* primary lung cancer".

(Case 12, which the clinico-pathology group labelled mediastinal malignancy *is* included, since the Epidemiologic-Statistical Subgroup indicates that such a case *is*, by coding rules, properly a respiratory malignancy.)

With the exclusion of the four cases above, there remain 45 cases for analysis, and these are presented by uranium-exposure subgroup in Table 2.

We can now proceed with an analysis of these cases, addressing ourselves to the questaion (*a*) through (*e*) above.

II. ESTIMATION OF DOUBLING DOSES IN WLM FOR RADON-DAUGHTER INDUCED LUNG CANCER

The first task is to determine the overall estimation of doubling dose in WLM units for lung cancer induction by radon-daughters. Then the task will be to determine how the dose-effect curve appears. This latter can be translated into the question of whether the doubling dose in WLM is rising, falling, or remaining constant over a large span of dosages.

The *first* requirement is an estimation of the "expected" number of lung cancers in each exposure category, corrected for cigarette smoking and other possible variables. Such an estimation is provided in Table 8 of (p. 1040), Reference 4, and is reproduced here:

TABLE 3

Exposure category	"Expected" number of cancers: 5 to 9 years	"Expected" number of cancers: Greater than 10 years	"Expected" total for persons 5 or more years after start of mining
A	0.48	0.37	0.85
B	.43	.50	.93
C	.36	.79	1.15
D	.22	.77	.99
E	.08	.58	.66
F	.02	.13	.15

Two bases for increasing these estimates are to be considered:

(*a*) **A few cases of lung cancer occurring after the June 1965 cut-off date are included in Table 2. These deaths occurred several months or so after the cut-off data. It is unimaginable that this should require**

more than a 10% correction in the "expected" cancer. But to be conservative, we shall give it a *20%* correction.

(*b*) The Epidemiologic-Statistical Subgroup expressed concern that the cigarette smoking effect might be 20% or even 40% greater than they used in deriving the "expected" cases. Following our analysis, using (*a*) a 20% greater value than the "expected", we shall also do the analysis allowing a *100% greater* "expected" value (much more than the 20–40% concern) and show that *none* of the conclusions concerning radiation-induction are substantively altered. Shown below are, therefore, the "expected" values, (*a*) Corrected by 20% and (*b*) corrected by 100%.

TABLE 4.—CORRECTED LUNG CANCER EXPECTATIONS IN BASE POPULATION FOR THOSE 5 OR MORE YEARS AFTER BEGINNING OF EXPOSURE

Exposure category	Expected number of cancers corrected by 20 percent (a)	Expected number of cancers corrected by 100 percent (b)
A	1.02	1.70
B	1.12	1.86
C	1.38	2.30
D	1.19	1.98
E	.79	1.32
F	.18	.30
Total group	5.68	9.46

(*a*) *Using the "Expectations" of Column* (*a*)

The analysis starts with a consideration of the doubling dose in WLM for the entire group of miners (Categories A through F) and proceeds serially by elimination, successively, of the high exposure groups. The goal, of course, is to determine what is happening to the WLM doubling dose in the lower dosage categories—the region of relevance for setting standards for "safe" mining exposure.

The overall group of 1981 miners

45 lung cancers observed
5.68 lung cancers "expected"

39.32 excess lung cancers in miners

$\frac{39.32}{5.68}=6.93$ doubling doses is what the excess cases of lung cancer show.

Now, to calculate the Mean Radiation dose in WLM, we proceed as follows:

$$\text{Mean Dose}=\frac{\left[\begin{array}{l}(383)(60)+(421)(240)+(496)(600)\\+(400)(1320)+(218)(2760)+(63)(6000)\end{array}\right]}{1981}$$

or

$$\text{Mean Dose}=\frac{22980+101040+297600+528000+601680+378000}{1981}=\frac{1,929,300}{1981}$$

or

Mean Dose=973.9 WLM

Therefore, *one* doubling dose$=\frac{973.9}{6.93}=$140.5 WLM

The group of miners, excluding group F, the highest exposure group

34 lung cancers observed (Groups A+B+C+D+E)
5.50 lung cancers "expected". Table 4(a) (Groups A+B+C+D+E)

28.50 excess lung cancers in Groups A through E inclusive

$\frac{28.50}{5.50}=$*5.18 doubling doses* is what the excess cases of lung cancer show.

For Mean Radiation dose, we have, for 1918 miners in A through E, inclusive:

$$\text{Mean Dose}=\frac{\left[\begin{array}{l}(383)(60)+(421)(240)+(496)(600)\\+(400)(1320)+(218)(2760)\end{array}\right]}{1918}$$

or

$$\text{Mean Dose}=\frac{22980+101040+297600+528000+601680}{1918}=\frac{1551300}{1918}$$

or

Mean Dose=808.8 WLM

Therefore, one doubling dose$=\frac{808.8}{51.8}=$156.1 WLM

The group of miners, excluding E+F, the two highest exposure groups.

25 lung cancers observed (Groups A+B+C+D)
4.71 lung cancers "expected" Table 4(a) (Groups A+B+C+D)

20.29 excess lung cancers in Groups A through D, inclusive.

$\frac{20.29}{4.71}=$*4.31 doubling doses* is what the excess cases of lung cancer show.

Now, for 1700 miners,

$$\text{Mean Dose}=\frac{(383)(60)+(421)(240)+(496)(600)+(400)(1320)}{1700}$$

or

$$\text{Mean dose}=\frac{22980+101040+297600+528000}{1700}=\frac{949620}{1700}$$

1128704

or

Mean Dose=558.6 WLM

Therefore, *one* doubling dose$=\frac{558.6}{4.31}=$129.6 WLM

The group of miners, excluding groups D, E, F, the three highest exposure groups

14 lung cancers observed (Groups A+B+C)
3.52 lung cancers "expected" Table 4(a) (Groups A+B+C)

10.48 excess lung cancers in Groups A through C, inclusive.

$\frac{10.48}{3.52}=$*2.98 doubling doses* is what the excess cases of lung cancer show.

For mean radiation dose in 1300 miners,

$$\text{Mean Dose}=\frac{(383)(60)+(421)(240)+(496)(600)}{1300}=$$

or

$$\text{Mean Dose}=\frac{22980+101040+297600}{1300}=\frac{421620}{1300}$$

or

Mean Dose=324.3 WLM

Therefore, one doubling dose$=\frac{324.3}{2.98}=$108.8 WLM

The group of miners, including A+B, the two lowest exposure categories

9 lung cancers observed (Groups A+B)
2.24 lung cancers "expected" Table 4(a) (Groups A+B)

6.76 excess lung cancers in Group A+B

$\frac{6.76}{2.24}=$3.02 doubling doses is what the excess cases of lung cancer show.

For mean radiation dose in 804 miners,

$$\text{Mean Dose}=\frac{(383)(60)+(421)(240)}{804}=\frac{22980+101040}{804}=\frac{124020}{804}$$

or

Mean Dose=154.3 WLM

Therefore, one doubling dose$=\frac{154.3}{3.02}=$51.1 WLM

Group A alone, the lowest exposure category

3 lung cancers observed (Group A)
1.02 lung cancers "expected" Table 4(a) (Group A)

1.98 excess lung cancers in Group A.

$\frac{1.98}{1.02} = $ *1.94 doubling doses* is what the excess cases of lung cancer suggest (small numbers preclude assurance)

For 383 miners, the radiation dose is as follows:

$$\text{Mean Dose} = \frac{22980}{383} = 60 \text{ WLM}$$

$$\text{Therefore, one doubling dose} = \frac{60}{1.98} = \underline{\underline{30.3 \text{ WLM}}}$$

We cannot assign too much assurance to this value since the total number of cancers (3) is so small as to have a high probable error. Of one thing we can be certain, the analysis of Group A alone, or Group A+B suggests the radiation risk is *growing*, if anything, *not* decreasing, in the low exposure categories. However, there *is* one correction that should be made for these lowest exposure categories before we seriously attempt to consider an increasing hazard at the lowest dosages. We shall now make this correction to the analysis.

In FRC 8 (Revised), account is taken of the fact that the lowest exposure categories deserve correction of WLM dose because of a contribution of (non-uranium) hardrock mining (6). This is important.

To take this into account, we shall triple the exposure for the lowest category (A), that is a change from 60 to 180 WLM, and we shall provide a 50% increase in dosage for category (B), that is a change from 240 to 360 WLM. The data of Reference 5 indicate these two categories are the only significant ones affected, and that our increases are *very* generous.

Recalculation of Group (A+B) crediting the hardrock mining exposure

9 lung cancers observed (Groups (A+B)
2.24 lung cancers "expected" Table 4(a) (Groups A+B)

6.76 excess lung cancers

$\frac{6.76}{2.24} = 3.02$ doubling doses.

Mean radiation dose (including correction for hardrock mining),

Mean Dose=

$$\frac{(3)(383)(60)+(1.5)(421)(240)}{804} = \frac{68940+151560}{804} = \frac{220500}{804}$$

or

Mean Dose = 274.3 WLM

$$\text{Therefore, one doubling dose} = \frac{274.3}{3.02} = 90.8 \text{ WLM}$$

SUMMARY OF ALL DOUBLING DOSE ESTIMATES AND THE QUESTION OF THRESHOLDS

Exposure category	Exposure mean (WLM)	Radiation doubling dose, lung cancer (WLM)
A+B+C+D+E+F	973.9	140.5
A+B+C+D+E	808.8	156.1
A+B+C+D	558.6	129.6
A+B+C	324.3	108.8
A+B (corrected for hardrock mining)	274.3	90.8

Inspection indicates that *all* the evidence points to radiation-induction of lung cancers, either with no trend in doubling dose, or if anything, a *trend toward lower doubling* doses at the low end of the dosage scale. Therefore, this is a confirmation of linear theory, or even possibly a demonstration that linear theory *underestimates* the risk at low total dosages.

Let us ask ourselves, in terms of doubling doses, what a so-called "threshold" means. *If* a "threshold" exists, this means that below such a "threshold" the radiation-cancer doubling dose must go to *infinity*. But the data above show that, with decreasing exposure dose, the doubling dose is *decreasing*, if anything—hardly a behavior of a function that is about to turn around and go to infinity! It is a strange phenomenon, indeed, to consider the "threshold hopers". No evidence whatever, for *any* form of cancer, has even *suggested* a threshold dose of radiation to be "safe" with respect to carcinogenesis in man. The claims of so-called "practical" or absolute thresholds are readily refuted (7)(8). In a public health problem of the gravity of setting radiation standards, and where the evidence indicates linearity (or an even greater than linear hazard at low doses) in a measurable region, one should shudder at the audacity of a man who announces, "Maybe, somehow, somewhere, someday, someone will show a threshold for *some* form of carcinogenesis". In this area of public health, the *burden of proof* is on whoever thinks there *might* be a threshold. The burden is *not* upon those who see no evidence for a threshold. Standards should never be set, counting upon mythical assistance to prevent damage to humans, particularly not for damage being the very destruction of human life. The Federal Radiation Council, in FRC 8, paid lip service to linearity and no threshold and then, in spite of warnings by Archer (9), Parker (10), Snyder (11), Morgan (12), and others, went ahead to set acceptable working levels that would double lung cancer for miners working 10 years. We shall return to this later.

(*b*) *Estimation of Doubling Doses Using the "Expectation" of Lung Cancer in Table 4*(*b*)

Table 4(b) provides a tabulation of "expected" lung cancer risks *double* that of the Epidemiology Group estimates. Thus cigarette smoking and any other biases are very generously over-compensated. Now let us see if this alters the picture for the behavior of the radiation-induced doubling dose for lung cancer.

The overall group of miners (A through F, inclusive)

45 lung cancers observed
9.46 lung cancers expected Table 4(b)

35.54 excess lung cancers

$$\frac{35.54}{9.46}=3.76 \text{ doubling doses.}$$

We have previously estimated mean dose=973.9 WLM

Therefore, one doubling dose= $\frac{973.9}{3.76}=\underline{259\ WLM}$

The group of miners (A through E) excluding the highest dose category.

34 lung cancers observed (Groups A+B+C+D+E)
9.16 lung cancers expected Table 4(b)

24.84 excess lung cancers

$$\frac{24.84}{9.16}=2.71 \text{ doubling doses.}$$

We have previously estimated the radiation dose=808.8 WLM

Therefore, one doubling dose=$\frac{808.8}{2.71}=\underline{\underline{298.5 \text{ WLM}}}$

The group of miners (A through D) excluding the 2 highest dose categories

25 lung cancers observed
7.84 lung cancers expected (Table 4(b))

17.16 excess lung cancers

$$\frac{17.16}{7.84}=2.19 \text{ doubling doses.}$$

We have previously estimated the radiation dose=558.6 WLM

Therefore, one doubling dose=$\frac{558.6}{2.19}=\underline{\underline{255.0 \text{ WLM}}}$

The group of miners (A+B+C) – the three lowest dose categories

14 lung cancers observed (Groups A+B+C)
5.86 lung cancers "expected", Table 4(b)

8.14 excess lung cancers

$$\frac{8.14}{5.86}=1.37 \text{ doubling doses.}$$

We have previously estimated the radiation dose=324.3 WLM

Therefore, 1 doubling dose=$\frac{324.3}{1.37}=\underline{\underline{236.7 \text{ WLM}}}$

The group of miners (A+B) – the two lowest categories

9 lung cancers observed (Groups A+B)
3.56 lung cancers "expected" Table 4(b)

5.44 excess lung cancers

$$\frac{5.44}{3.56}=1.52 \text{ doubling doses.}$$

We have estimated the radiation dose above, *including* a generous correction for hardrock mining exposure, to be=274.3 WLM

Therefore, 1 doubling dose=$\frac{274.3}{1.52}=\underline{\underline{180.5 \text{ WLM}}}$

Now let us summarize all these calculations:

Exposure category	Exposure mean WLM	Radiation doubling dose, lung cancer (WLM)
A+B+C+D+E+F	973.9	259.0
A+B+C+D+E	808.8	298.5
A+B+C+D	558.6	255.0
A+B+C	324.3	236.7
A+B (corrected for hardrock mining)	274.3	180.5

Again, even with the *exaggerated* correction of Table 4(b) for cigarette smoking or other possible bias, the answer is the same—perfect agreement with linear theory (=constancy of doubling dose) over the entire range of doses, with the lowest dose category suggesting an increased *hazard* over linear theory, if anything. Recall again, so-called "threshold" theory would require doubling dose to trend toward *infinity* in the low dose categories. Inspection of the actual trend readily shows the absurd nature of such "threshold hoping".

Morgan, reading from the FRC Advisory Committee Report ("Radiation Exposure to Uranium Miners", August 1968) quotes, "At each level of radiation exposure, including the level equal to and less than 120 working level months, in the uranium mines a significant excess of respiratory cancer has now been observed among the white miners. Two exposure levels are now significant for the first time". (13)

Archer has predicted this in the JCAE Hearings based upon his analysis of the then existing data (14).

In the analysis of *this* report, based upon data available by 1967, we concluded that everything pointed unmistakably to a clear-cut association of respiratory cancer for the lowest two working levels. While for rigorous epidemiological purposes, cases were PROPERLY excluded by the Epidemiologic-Statistical Subgroup, and thus making absolute

proof difficult for the lowest categories, our analysis demonstrates clearly that sufficient data *were* available to allow this conclusion in 1967—certainly more than adequate for considerations involving the health of the miners.

There *are* times when epidemiologic data can be over-restrictively interpreted. For rigor, this restrictive approach is beautiful. For protecting lives, there is no loss if the data are pressed fully for what they *soundly* can reveal. It appears there are already too many persons involved in standard-setting who demand seeing every possible coffin before they are galvanized into action.

III. THE TRUE DOUBLING DOSE FOR LUNG CANCER INDUCTION BY RADON DAUGHTERS

All the analyses above are based upon observed lung cancers in miners who are 5 or more years beyond the inception of their mining experience, with many of them less than 10 years since inception. Clearly we are still early in the latency period for many of them. Following Archer's general models, which assume a mean of 20 years from first exposure to appearance of lung cancer (5), we can feel highly confident that the 45 cases observed would have been considerably higher if these studies had been conducted at an interval longer than that concluded in June 1965. To be conservative, and not overestimate the situation, it is doubtful that anyone would argue that "at full bloom", 1.5 times as many cases would have been observed in this study conducted at a later time.

(45) (1.5) = 67.5 cases would have been observed.

Now let us estimate the doubling doses, using Table 4(a) "expected" values and Table 4(b) "expected" values.

	Conservative Calculation Table 4(a) "expected"	*Exaggerated Calculation* Table 4(b) "expected"
Category:	A+B+C+D+E+F	A+B+C+D+E+F
Mean Dose	973.9 WLM	973.9 WLM
Observed	67.5	67.5
Expected	5.68	9.46
Excess Cancers	61.82 cases	58.04
Doubling Doses	$\frac{61.82}{5.68}=10.88$	$\frac{58.04}{9.46}=6.13$
One Doubling Dose	$=\frac{973.9}{10.8}=$ 90.2 WLM	$=\frac{973.9}{6.13}=$ 154.6 WLM

For the sake of conservation, we shall choose a final value midway between these, or 122.4 WLM. We fully expect, as data accumulate, true value will lie *below* this value, and from every evidence, this doubling dose will hold *all the way* down to the lowest dose categories. Nor should this value surprise anyone, since Archer suggested the doubling dose to lie between 100–400 WLM with "a high probability it lies at the lower limit of this range" (14). We agree!

IV. THE TYPES OF LUNG CANCER INDUCED BY RADON DAUGHTER EXPOSURE

Saccomanno and co-workers discovered that some 57% of the lung cancers among United States uranium miners were of the small cell undifferentiated variety, many referred to as of "oat cell" variety. Among non-miners, small cell undifferentiated cancers were estimated rarely to exceed 20% of all lung cancers. (15) These appear to be quite firm, reliable data.

However, in the absence of a *quantitative interpretation* of these important findings, a lore has arisen which holds the remarkable view that *radiation* induces a specific histologic type of lung cancer. So widespread has this fiction become that Miller (16) and Storer (17) have attempted to use it to raise doubts about the induction of lung cancer by radiation in the Hiroshima-Nagasaki survivors. No more grave error could be committed in this area of human radiation biology.

Let us approach this problem by consideration of the 45 cases of lung cancer evaluated above, and let us use the "expected" lung cancers from Table 4(a). Using Saccomanno's estimates:

"Observed" = (0.57) (45) = 25.7 small cell, undifferentiated lung cancers.

"Observed" = (0.43) (45) = 19.3 bronchiogenic cancers.

"Expected" = (0.20) (5.68) = 1.14 small cell, undifferentiated lung cancers.

(0.80) (5.68) = 4.54 bronchiogenic cancers.

Therefore, for Bronchiogenic Cancer:

19.3 cases "observed" bronchiogenic cancers
4.54 cases "expected" bronchiogenic cancers

14.76 cases, *excess* bronchiogenic cancers

We doubt that even the most hardened biometrician would require formal statistics to prove that bronchiogenic cancer, from these data, *is* radiation-induced. It clearly is!

$$\frac{14.76}{4.54}=3.25 \text{ doubling doses.}$$

Mean radiation dose = 973.9 WLM

So, 1 doubling dose for *Bronchiogenic Cancer* $=\frac{973.9}{3.25}=299.7$ WLM

For Small Cell, Undifferentiated Cancer:

25.7 cases "observed" small cell, undifferentiated cancers
1.14 cases "expected" small cell, undifferentiated cancers

24.56 cases, *excess* small cell, undifferentiated cancers

Obviously, such cancers are also radiation-induced.

$$\frac{24.56}{1.14}=21.54 \text{ doubling doses.}$$

So, 1 doubling dose for *Small Cell, Undifferentiated Cancer*

$$=\frac{973.9}{21.54}=45.2 \text{ WLM}$$

These analyses prove that *two* types of cancer are induced by exposure to radon daughters in uranium miners, namely bronchiogenic cancer *and* small cell, undifferentiated cancer. The *six-fold* lower doubling dose for small cell cancer induction than for bronchiogenic cancer is, in all probability, *not* a real difference in susceptibility to cancer induction, but rather is a reflection of the *distribution* of the radon daughter irradiation—with the "small cells" receiving much more radiation than those cells which give rise to the bronchiogenic cancers. Too little is known of the behavior of the radon daughters to pin this down, but it seems a far more likely explanation than a difference in intrinsic susceptibility to radiation-induced cancer.

In any event, *both* bronchiogenic lung cancer *and* small cell, undifferentiated cancer are clearly radiation-induced. It would be helpful if the fiction of a specific radiation-induced type of lung cancer would die now, before it can cause further confusion and, thereby, damage to human lives.

V. THE IMPLICATIONS OF THE NEW FRC GUIDELINES FOR URANIUM AND HARDROCK MINING EXPOSURE TO RADON DAUGHTERS

After elegant testimony by Archer (14), Parker (9), and Snyder (11), as well as others, in the important JCAE Hearings, the Federal Radiation Council came up with the astounding conclusion that:

"a higher than expected incidence of lung cancer is demonstrated when the cumulative exposures are more than about 1000 WLM. The degree of risk at lower levels of cumulative exposure cannot be determined from currently available epidemiologic data."(6)

There would appear to exist no rational way to explain these statements. The same data were *then* to the Federal Radiation Council as are available to us now for the analyses completed above. Our analyses showed clearly that radiation-induction appears to have an essentially constant doubling dose (agreement with linear theory) down to the lowest two dosage categories. The *deviation*, if significant, in the low dose categories was in the direction of a *higher* risk of cancer induction; *not* lower. (See entire analysis in preceding sections). So there was clear-cut evidence available for radiation induction way, way below the 1000 WLM spoken of by the FRC. But Snyder *did* do an analysis and *did* point out that there was *no reason to believe* that effects upon the miners did not extend down to the lowest dose range(11). The words of Snyder at that Hearing are so beautiful and so relevant on this issue that they must be quoted here (11): (Snyder) "However, one may wonder why it is considered so undesirable to use a conservative criterion where human life is in question. Surely if the linear hypothesis is conservative and is not in conflict with the data that are available, this is a point in its favor. When human life is in the balance, it would seem that conservatism in safeguarding those lives has much to commend it."

Eloquent, scientifically sound words, in the best public health tradition! Who was listening?

VI. JOACHIMSTHAL REVISITED? THE VERY SERIOUS PROBABLE CONSEQUENCES OF THE NEW FRC GUIDELINES FOR URANIUM MINING

That we, in the United States, have repeated much of the tragedy of Joachimsthal and Schneeburg in the period from 1940 through 1967 is indeed regrettable. But human errors *do* occur, and it is best to forgive those that are past. However, if we go forward with a new generation of miners and repeat the Joachimsthal experience *again*, this will be absolutely inexcusable.

As we shall show below, it is our contention that the revised FRC Guidelines for uranium mining exposure to radon daughters (12 WLM per year) has a dangerously high probability of a needless repetition of the Joachimsthal tragedy (Even the *more* recent 3.6 WLM per year of the Labor Department is high).(18)

The hazards in the FRC Guidelines are in two parts:

(a) A mis-estimation by the FRC of the meaning of a doubling of the lung cancer risk in comparison with the risk of a fatal accident in mining. The FRC error is *ten*-fold.

(b) A far more serious possibility that resides in the lack of appreciation of the significance of the fact that *both bronchiogenic and small cell undifferentiated cancer are radiation-induced.* This possibility, as we shall show, can have the most dire consequences, and of itself should certainly lead to a drastic lowering of the Mining Guidelines.

We shall consider (a) and (b) serially now.

(a) *The FRC Mis-estimation of the Lung Cancer Risk vs Fatal Accidents*

In FRC 8 (Preliminary, May 1967) (Paragraph 3.35), the FRC presents "a general perspective on the magnitude of the lung cancer risk for a stated occupational exposure can be gained by comparing it to the risk of a fatal accident in the same occupation". The FRC goes on to estimate that:

(a) For 1000 men working 10 years, there will be 20 deaths from fatal accidents.

(b) For 1000 men exposed to an average of 2× the lung cancer risk over a 10-year period, they would be expected to incur 2 lung cancers.

(c) The FRC states, "It appears that a lung cancer risk twice expected would be about one-tenth the risk of a fatal accident".

Concerning:

(a) We do not disagree about the 20 deaths from accidents.

(b) We disagree violently about how the FRC estimates the meaning of doubling the lung cancer risk.

(c) The final FRC estimate is erroneous by at least a factor of 10.

In order to demonstrate the FRC error we need to go through a few simple calculations.

Let us start with a group of 1000 men at age 20 years who enter the occupation of uranium mining *de novo*.

Let these men work 10 years at FRC Guidelines, during which they would accumulate one doubling dose of radon daughters. (FRC Guidelines allow 120 WLM; our estimate is that this is more than one doubling dose, but for consistency with the FRC "perspective" we shall accept this as one doubling dose).

Further, we shall assume *no* cancers at all during the ten-year period of mine exposure, from age 20 to age 30 years. However, by age 40 years, we shall assume (approximately following Archer) that lung cancers occur, and that between 40–50 years of age, the rate is ½ that expected at full effect of the radiation.

Between 50 and 70 years, we shall assume that the full effect of the radiation is experienced each year. Thus, between 50 and 70 years, one doubling dose will produce a number of additional cancers equal to the spontaneously occurring number.

First we need data concerning the spontaneous occurrence rate for lung cancers for the age span 40–70 years. From Hammond's data, we calculate the following expectancies for smokers of 10–19 cigarettes/day (19). (The miners average ≅13/day. (Ref. 5, p. 1277)

Age (males:)	Lung Cancers/ 100,000/year
45 years	24
50 years	65
55 years	106
60 years	147
65 years	183
70 years	208

Next, we need to know how many men survive to be at risk at each age from 40 to 70 years. Using the U.S. Vital Statistics for 1966, we have calculated the number of men surviving at various ages, starting with 100,000 men at age 20 years (20). These are listed below.

Survivors at various ages starting with 100,000 men at age 20 yrs. (U.S. males)

Age—	Survivors at specified ages
at 24 years	99,000
at 29 years	98,069
at 34 years	96,990
at 39 years	95,535
at 44 years	93,270
at 49 years	89,749
at 54 years	84,256
at 59 years	76,135
at 64 years	65,402
at 69 years	51,344
at 74 years	35,522
at 79 years	20,398

Now to calculate the lung cancers, starting to occur at age 40 years in our miners exposed to one doubling dose.

40–50 yr. decade 10 years
~91.5% have survived to be at risk
½ of full effect occurs in this decade (0.5 doubling)
24/100,000 is the spontaneous rate of lung cancer

$$\therefore (0.915)(0.5)\times\frac{24}{100000}\times 1000\times 10=\frac{109800}{100000}=1.1 \text{ cases}$$

50–60 yr. decade 10 years
~80.2% have survived to be at risk
Full effect occurs in this decade (doubling dose)
106/100,000 is the spontaneous rate of lung cancer

$$\therefore (0.802)\times\frac{106}{100000}\times 1000\times 10=8.5 \text{ cases}$$

60–70 yr. decade 10 years
~58.4% have survived to be at risk
Full effect occurs in this decade (doubling dose)
183/100,000 is the spontaneous rate of lung cancer

$$\therefore (0.584)\times\frac{183}{100000}\times 1000\times 10=10.7 \text{ cases}$$

Thus, if we neglect radiation-induced cancers beyond 70 years (*not* a negligible number), we can add up 1.1+8.5+10.7=*20.3* cases of lung cancer per 1000 men mining uranium at one doubling dose accumulated over a 10-year period.

$$\text{For 1000 men}\begin{cases}\text{Thus, fatal accidents in 10 years}=20\text{ deaths}\\\text{Lung cancer, from 10 years of mining}=20.3\text{ deaths.}\end{cases}$$

Therefore, these two sources of death are equal. To be sure the radiation-induced lung cancers occur later in life, but the number *equals* the accidental death number, rather than being 0.1 as high, as claimed by FRC 8.

(*b*) *The Serious Implications of the Two Types of Cancers Induced by Radon Daughters*

In Section IV it was demonstrated that Saccomanno's finding of a different proportion of bronchiogenic versus small cell cancer in uranium miners compared with non-miners was extremely important. First, the use of his evidence allowed a demonstration that *both* bronchiogenic and small cell cancer are radiation-induced. Second, the use of his data allowed a comparison of doubling doses for the two forms of lung cancer. These were:

299.7 WLM for bronchiogenic cancer
45.2 WLM for small cell, undifferentiated cancer.

We indicated there that we believed it far more likely that different *dosages* to the relevant tissues accounted for the difference in *apparent* doubling dose, rather than a different intrinsic susceptibility to cancer induction. We shall now treat this issue in extenso, for it has major implications for the uranium miners.

Recently we presented three major laws of radiation-carcinogenesis in man (21). The second of those three laws states:

"All forms of cancer show closely similar doubling doses and closely similar increases in incidence per rad".

We shall now demonstrate, in practice, how useful these laws can be in understanding a problem such as radon-daughter exposure.

Now, we have an *apparent* 6.6-fold higher doubling dose for bronchiogenic cancer than for small cell cancer (299.7/45.2). We shall use Law 2, and *state*:

"The doubling dose for bronchiogenic cancer *equals* that for small cell cancer".

How, then shall we explain the 6.6-fold *apparent* difference? We shall accomplish this by consideration of dosage to the relevant target tissues.

Before going ahead, we shall refer to Section III, where in discussing the "true" doubling dose, we pointed out that the 45 cases in 1981 miners do *not* represent the full effect because of latency, and that an estimate of 67.5 cases for 1981 miners is *not* an overestimate of the true value.

Further, we shall use the data for all 1981 miners because we have seen that doubling doses estimated for the overall group understate the problem rather than overstating it. So, we have the following parameters:

Categories: A+B+C+D+E+F
No. of miners: 1981
Mean Dose: 973.9 WLM units
"Observed": 67.5 Lung cancers (all types) (corrected for latency)
"Expected": 5.68 Lung cancers (all types) Table 4(a)
Excess: 61.82 Lung cancers (all types)

Overall Doubling Doses: $\frac{61.82}{5.68}=10.88$ doubling doses.

From Saccomanno (15), we have:
In "spontaneous" lung cancers (including cigarette smoking)
80% bronchiogenic
20% small cell, undifferentiated
In uranium miner lung cancers
43% bronchiogenic
57% small cell, undifferentiated.

What we do *not* know is the size of the *two domains*, (a) that which gives rise to bronchiogenic cancer and (b) that which gives rise to small cell, undifferentiated cancer.

Let us suppose there are no *other* domains exposed, and that the full dose 973.9 WLM is distributed between these two domains.

Let the total mass of both domains be arbitrarily set at *unity*.

Let A be the fractional mass of the "bronchiogenic" domain.

Let B be the fractional mass of the "small cell" domain.

Let the total WLM be distributed in some manner (to be calculated) between A and B.

Now,

(I) A+B≡1
(II) Dosage can be expressed in the units:

$$\frac{(WML)_A}{A} \text{ and } \frac{(WLM)_B}{B}$$

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(III) Let D=the identical doubling dosage for cancer induction both domains.

Utilizing our input parameters, we have
For bronchiogenic cancers, (0.43) (67.5) =29.0 "observed"
(0.80) (5.68) = 4.54 "expected"
Excess = 24.46 cases

$$\text{Doubling doses}=\frac{24.46}{4.55}=5.39$$

(IV) So $\left[\frac{(WLM)_A}{A}\right]=(5.39)(D)$

For small cell cancers, (0.57) (67.5) =38.5 "observed"
(0.20) (5.68) = 1.14 "expected"
Excess = 36.36 cases

$$\text{Doubling doses}=\frac{36.36}{1.14}=31.89$$

(V) So, $\left[\frac{(WLM)_B}{B}\right]=(31.89)(D)$

Therefore,

(VI) $$\frac{\left[\frac{(WLM)_A}{A}\right]}{\left[\frac{(WLM)_B}{B}\right]}=\frac{(5.39)(D)}{(31.89)(D)}$$

or, eliminating D, we have

(VII) $$\left[\frac{(WLM)_B}{B}\right]=\left(\frac{31.89}{5.39}\right)\left[\frac{(WLM)_A}{A}\right]=(5.92)\left[\frac{(WLM)_A}{A}\right]$$

We have arrived then at the knowledge that the dose in (WLM) per unit mass of domain is *5.92* times as high in the small cell domain as in the bronchiogenic domain. But we still do not know A or B, so we cannot know how to apportion the total WLM into $(WLM)_A$ and $(WLM)_B$. Within these data, we cannot ascertain A and B. Instead we shall consider the implications of 3 possibilities of the size of these domains.

(a) $A=B$ (of course $A+B=1$)
(b) $A=9(B)$, which means $B=0.1$, $A=0.9$
(c) $B=9(A)$, which means $B=0.9$, $A=0.1$

Now we can proceed to calculations of D for these 3 possibilities.

(a) *Calculation of* "D", *where* A=B=*0.5*

$$\text{Overall Dose}=\frac{\text{Overall } WLM}{1.0}$$

$$\text{Bronchiogenic Domain Dose}=\frac{(WLM)_A}{A}=\frac{(WLM)_A}{0.5}$$

$$\text{Small Cell Domain Dose}=\frac{(WLM)_B}{B}=\frac{(WLM)_B}{0.5}$$

But from (VII), we have

$$\left[\frac{(WLM)_B}{B}\right]=(5.92)\left[\frac{(WLM)_A}{A}\right]$$

Substituting A=B=0.5, we have

$$\frac{(WLM)_B}{0.5}=(5.92)\frac{(WLM)_A}{0.5}$$

or

$$(WLM)_B=5.92\ (WLM)_A.$$

But,

$$\text{Total } WLM=(WLM)_A+(WLM)_B$$

So, substituting, $973.9=(WLM)_A+(5.92)(WLM)_A$

or $(6.92)(WLM)_A=973.9$

$$(WLM)_A=\frac{973.9}{6.92}=140.7\ WLM \text{ units}$$

$$(WLM)_B=973.9-140.7=833.2\ WLM \text{ units}$$

Therefore,

$$\frac{(WLM)_A}{A}=\frac{140.7}{0.5}=281.4\ WLM \text{ units/unit mass of domain}$$

$$\frac{(WLM)_B}{B}=\frac{833.2}{0.5}=1666.4\ WLM \text{ units/unit mass of domain}$$

We now can calculate D, the doubling dose, either from the bronchiogenic or the small cell data. Since we have assumed a *single* value of D for both domains, we must get the same answer, if all the arithmetic is correct above.

For bronchiogenic cancer data, we have 5.39 doubling doses. Therefore:

$$\frac{281.4}{5.39}=D=52.2\ WLM \text{ units/unit mass}$$

For the small cell cancer data, we have 31.89 doubling doses. Therefore:

$$\frac{1666.4}{31.89}=D=52.3\ WLM \text{ units/unit mass}$$

We can say $D=52.2$ WLM units/unit mass in either domain.
Note: According to the assignment of $A=B=0.5$, 833.2 WLM units are in the small cell domain and 140.7 WLM units are in the bronchiogenic domain. This accounts for the smaller excess cancers in the bronchiogenic domain than in the small cell domain, *even* though D is identical in both domains.

(*b*) *Calculation of "D", where $A=9\ B$ ($A=0.9$; $B-0.1$)*

$$\text{Overall dose}=\frac{\text{overall } WLM}{1.0}$$

$$\text{Bronchiogenic domain dose}=\frac{(WLM)_A}{A}=\frac{(WLM)_A}{0.9}$$

$$\text{Small cell domain dose}=\frac{(WLM)_B}{B}=\frac{(WLM)_B}{0.1}$$

But from (*VII*), we have

$$\left[\frac{(WLM)_B}{B}\right]=(5.92)\left[\frac{(WLM)_A}{A}\right]$$

Substituting $A=0.9$, $B=0.1$, we have

$$\frac{(WLM)_B}{0.1}=(5.92)\frac{(WLM)_A}{0.9}$$

or

$$(WLM)_B=\frac{(5.92)}{9.0}(WLM)_A=(0.66)(WLM)_A$$

But,

$$\text{Total } WLM=(WLM)_A+(WLM)_B$$

$$973.9=(WLM)_A+(0.66)(WLM)_A$$

or $1.66\ (WLM)_A=973.9$

$$(WLM)_A=\frac{973.9}{1.66}=586.7\ WLM \text{ units}$$

$$(WLM)_B=973.9-586.7=387.2\ WLM \text{ units}$$

Therefore,

$$\left[\frac{(WLM)_A}{A}\right]=\frac{586.7}{0.9}=651.9\ WLM \text{ units/unit mass}$$

$$\left[\frac{(WLM)_B}{B}\right]=\frac{387.2}{0.1}=3872.0\ WLM \text{ units/unit mass}$$

Now to calculate "D" for both domains:
For bronchiogenic cancer data, we have 5.39 doubling doses
Therefore:

$$\frac{651.9}{5.39}=D=120.9\ WLM \text{ units/unit mass}$$

For small cell cancer data, we have 31.89 doubling doses
Therefore,

$$\frac{3872.0}{31.89}=D=121.4\ WLM \text{ units/unit mass}$$

Say $D=121.2$ WLM units/unit mass in either domain.
Note: According to this assignment $A=0.9$ and $B=0.1$, more WLM units (586.7) are in A domain than in B domain (387.2). But because of the larger mass of A ($9 \times B$), the dose per unit mass is still much lower in A than in B, which accounts for the smaller number of excess cancers in the bronchiogenic domain than in small cell domain. D, again, is identical in *both* domains.

It is important to note that if $A=9B$ in domain size, the doubling dose, D, of 121.2 WLM units/unit mass is much higher than $D=52.2$ WLM units/unit mass, where $A=B$. So, the true sensitivity of lung tissue for radiation-carcinogenesis is very strongly dependent on *domain sizes.*

(*c*) *Calculation of "D" where* B=9(A) (A=0.1; B=0.9)

$$\text{Overall Dose} = \frac{\text{Overall } WLM}{1.0}$$

$$\text{Bronchiogenic Domain Dose} = \frac{(WLM)_A}{A} = \frac{(WLM)_A}{0.1}$$

$$\text{Small Cell Domain Dose} = \frac{(WLM)_B}{B} = \frac{(WLM)_B}{0.9}$$

But from (VII), we have

$$\left[\frac{(WLM)_B}{B}\right] = (5.92)\left[\frac{(WLM)_A}{A}\right]$$

Substituting $A=0.1$, $B=0.9$, we have

$$\left[\frac{(WLM)_B}{0.9}\right] = (5.92)\left[\frac{(WLM)_A}{0.1}\right]$$

or

$$(WLM)_B = (9)(5.92)(WLM)_A$$
$$(WLM)_B = (53.28)(WLM)_A$$

But,

$$\text{Total } WLM = (WLM)_A + (WLM)_B$$
$$9.73 = (WLM)_A + (53.28)(WLM)_A$$

or $54.28(WLM)_A = 973.9$

$$(WLM)_A = \frac{973.9}{54.28} = 17.9\ WLM \text{ units}$$

$$(WLM)_B = 973.9 - 17.9 = 956.0\ WLM \text{ units}$$

Therefore,

$$\left[\frac{(WLM)_A}{A}\right] = \frac{17.9}{0.1} = 179\ WLM \text{ units/unit mass}$$

$$\left[\frac{(WLM)_B}{B}\right] = \frac{956.0}{9.9} = 1062.2\ WLM \text{ units/unit mass}$$

Now to calculate "*D*" for both domains:

For bronchiogenic cancer data, we have 5.39 doubling doses
Therefore,

$$\frac{179}{5.39} = D = 33.2 \text{ WLM units/unit mass}$$

For small cell cancer data, we have 31.89 doubling doses
Therefore,

$$\frac{1062.2}{31.89} = D = 33.3 \text{ WLM units/unit mass}$$

So, we can say D=33.3 WLM units/unit mass

Note: *How* VERY *low* the *true* doubling dose for radiation-induction of cancer is if we assume the bronchiogenic domain mass (A) to be

1/9 as large as the small cell domain mass (B). And, we simply *don't* know the size of these two domains, so *this low a doubling dose is quite possibly the correct one!*

Summary

Domain Size		*Doubling Dose, D, for Lung Cancer*
A=0.9	B=0.1	121.2 WLM units/unit mass
A=0.5	B=0.5	52.2 WLM units/unit mass
A=0.1	B=0.9	33.3 WLM units/unit mass

The doubling dose is highly dependent on assumed A and B, neither of which we know. Worse yet, the implications are vastly different, even beyond the D value itself, for the three different situations. Let us turn to such implications now.

(*c*) *Some Possible Serious Surprises in the Future of Uranium Miner Lung Cancer*

We shall now explore the implications of the assumed tissue domain sizes and D values for what may happen in the uranium mines. The factors which determine, in a particular person, in a particular mine, in respect to humidity in the mine, in respect to dust composition in the mine,—what the *distribution* of radon daughters will be into the two domains are so poorly known that we can say they are *unknown.* As we shall see below, if a change in these factors occurred in a particular mine, or if we consider the possibility of miner to miner variation, the implications can be drastic.

Let us consider serially, the effect of a *different* distribution of WLM into the two domains from what currently seems to be the average for uranium miners studied thus far. We shall explore the implications of *equal* distribution of total WLM into the two domains.

Start with case (*b*), *where* $A=9B$ ($A=0.9$; $B=0.1$) D=121.2 WLM

In the calculations above, for these domain sizes, we have

$$(WLM)_A = 586.7 \text{ WLM units}$$
$$(WLM)_B = 387.2 \text{ WLM units}$$

Let us suppose, either because of miner variation, or condition differences in the mines, that a shift in distribution occurred to

$$(WLM_A = (WLM)_B = \tfrac{1}{2}\ (973.9) = 487 \text{ WLM units}$$

Bronchiogenic Domain Dose becomes

$$= \frac{(WLM)_A}{A} = \frac{487}{0.9} = 541 \text{ WLM units/unit mass}$$

Small Cell Domain Dose becomes

$$= \frac{(WLM)_B}{B} = \frac{487}{0.1} = 4870 \text{ WLM units/unit mass}$$

For bronchiogenic cancer, excess $\frac{541}{121.2} = 4.46$ doubling doses

Since expected = 4.54 cancers (See above III)
The Excess = (4.54) (4.46) = 20.25 cases
Total bronchiogenic cancers = Spontaneous + Radiation-Induced
= 4.54 + 20.25 = 24.79 cases

For small cell cancer, excess $= \frac{4870}{121.2} = 40.18$ doubling doses

Since expected = 1.14 cancers
Excess cases = (1.14) (40.18) = 45.81
Total small cell cancers = Spontaneous + Radiation-Induced
= 1.14 + 45.81 = 46.95 cases
Total Cancers, both types = 24.79 + 46.95 = 71.74 cases

This is to be compared with 67.5 for the *existing* dose distribution.

Therefore, the shift in dosage distribution is *not* serious since there will be only a $\frac{71.74}{67.5} =$ 1.06-fold increase in cancer incidence.

Now try (a), where A = B (A = 0.5; B = 0.5) D = 52.2 WLM

In the calculations above, for these domain sizes, we have

$(WLM)_A = 140.7$ WLM units
$(WLM)_B = 833.2$ WLM units

Let us now suppose a shift in distribution were to occur, so that

$(WLM)_A = (WLM)_B = 487$ WLM units

Bronchiogenic Domain Dose becomes $\frac{487}{0.5} = 973.9$ WLM units/unit mass

Small Cell Domain Dose becomes $\frac{487}{0.5} = 973.9$ WLM units/unit mass

For bronchiogenic cancer, excess $= \frac{973.9}{52.2} = 18.7$ doubling doses

Since expected = 4.54 cancers
The Excess = (4.54) (18.7) = 84.9 cancers
Total bronchiogenic cancers = Spontaneous + Radiation-Induced
= 4.54 + 84.9 = 89.44 cases

For small cell cancers, excess $= \frac{973.9}{52.2} = 18.7$ doubling doses also

Since expected = 1.14 cases
Excess = (1.14) (18.7) = 21.3 cancers
Total small cancers = 1.14 + 21.3 = 22.44 cases
Total Cancers, both types = 89.44 + 22.44 = 111.88 cases

So, $\frac{111.88}{67.5} = 1.66$-fold increase in cancers

Lastly, try case (c), where B = 9A (A = 0.1; B = 0.9) D = 33.3 WLM

In the calculations previously, for these domain sizes, we have
$(WLM)_A = 17.9$ WLM units
$(WLM)_B = 956.0$ WLM units

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Let us now suppose, either because of miner variations or mining condition change, a shift occurred to
$(WLM)_A = (WLM)_B = 487$ WLM units

Bronchiogenic Domain Dose becomes $\frac{487}{0.1} =$
4870 WLM units/unit mass

Small Cell Domain Dose becomes $\frac{487}{0.9} =$
541.1 WLM units/unit mass

For bronchiogenic cancers, excess $= \frac{4870}{33.3} = 146.2$ doubling doses

Since expected = 4.54 cancers
The Excess = (454) (146.2) = 651.2 cancers
Total Cancers = Spontaneous + Radiation-Induced
= 4.54 + 651.2 = 655.74

For small cell cancers, excess $= \frac{541.1}{33.3} = 16.2$ doubling doses.

Since expected = 1.14 cancers
Excess = (1.14) (16.2) = 18.5 cancers
Total small cancers = Spontaneous + Radiation-Induced
= 1.14 + 18.5 = 19.64 cancers
Total Cancers, both types = 655.74 + 19.64 = 675.4 cases

$\frac{675.4}{67.5}$ = a *10-fold increase* in number of cancers

Thus, with this assumed size of domains (A = 0.1 and B = 0.9) with a shift in distribution of burden to equal amounts in each domain, *a 10-fold increase* in cancers could occur over whatever value is calculated from past experience for any exposure level. And we do *not* know that these domain sizes are not the true ones.

Thus, even with the grudging FRC lowering of guidelines in 1967 to 12 WLM/year, and even with more recent Labor Department reduction to 3.6 WLM/year, this still means 36 WLM in a 10-year mining period of exposure. If the domain sizes are as indicated, and either for certain miners or mine conditions, the distribution of burden ever became equal in the two domains, there could be a 10-fold increase in total cancer incidence over what anyone might have thought possible. And this would indeed represent Joachimsthal revisited.

There may be those who say this is not at all reasonable since Joachimsthal had 30–150 WLM concentrations of radon-daughters. *But* Joachimsthal also 30–70% of all miners die of lung cancer (4). Since *some* miners must have died of heart disease and other causes, the 30–70% deaths due to lung cancer can, for all practical purposes, represent massive overkill. It just might not have been possible to have more lung cancers because the supply of candidates ran out! Furthermore, no one knows that the conditions in Joachimsthal mines were at all similar to those in USA uranium mines, and how those

condition differences might affect burden distributions into the two domains.

These calculations should put those involved in setting standards on notice concerning what a treacherous problem this radon-daughter situation is in our primitive knowledge of the domain sizes and factors that *may* shift burden. Allowed levels of 12 WLM/year or 4 WLM/year may, with further experience, prove *far* from conservative. This problem deserves sober reconsideration with a possible view toward appreciable further lowering of the allowable level of radon-daughters to avoid revisiting Joachimsthal.

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Radiation-Induction of Human Breast Cancer

(By Arthur R. Tamplin and John W. Gofman)

[This is Document No. 6 in a Series of 7. Issued, Documents 3, 4]

INTRODUCTION

Mackenzie made some extremely important observations concerning an apparent association between the occurrence of breast cancer and prior pneumothorax therapy of pulmonary tuberculosis (1). His analysis led him to the conclusion that fluoroscopic X-ray exposure associated with the pneumothorax therapy might have been etiologic in the induction of breast cancer in women so exposed. Wanebo and co-workers were so impressed by the Mackenzie findings that they decided to make a specific study of breast cancer incidence in the survivors of Hiroshima-Nagasaki (2). Wanebo revealed an extremely important point concerning the studies of atom-bomb survivors, a point of deep importance that is not at all realized broadly. Let us quote Wanebo and co-workers directly,

"Unlike leukemia and thyroid cancer, breast cancer has hitherto received no special emphasis in the ABCC program".

The work of Mackenzie actually was a primary stimulus for Wanebo and co-workers to search out the breast cancer situation in Hiroshima-Nagasaki.

This is indeed a revelation. All cancers are not *automatically* searched out in the Hiroshima-Nagasaki study. Rather, when some relevant suggestion comes up concerning radiation carcinogenesis of a particular organ, a diligent search is *then* made among Hiroshima-Nagasaki survivors for evidence of a radiation-induction of *that* disease in the bomb survivors. Thus, eventually, it can be expected that all forms of cancer will be investigated adequately in Hiroshima-Nagasaki. These studies are crucial and invaluable! But what this sequence of events teaches us is that *at any point in time* the failure of apparent existence of radiation-induction of a particular cancer in Hiroshima-Nagasaki survivors *can* be more related to a failure *to look* at that time than to absence of radiation-induction. This is in no way a criticism of ABCC. Its work is truly of monumental importance. But this should, for once and all, silence those who say, "But radiation-induction of cancer X hasn't been observed in Hiroshima-Nagasaki survivors". The answer appears that either (a) the ABCC staff hasn't yet completed *studying* that cancer, or (b) the latent period for radiation-induction may be longer than for those cancers which have already been clearly proved to be radiation-induced. In time the ABCC will undoubtedly provide data concerning every major form of human cancer induction by ionizing radiation.

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THE MACKENZIE BREAST CANCER DATA

The first observation of Mackenzie was the occurrence of a breast cancer on the upper part of the inner half of the breast in a woman whose skin showed residual evidence of radiation-type dermatitis. The unusual location of the breast cancer plus the suspicion of radiation changes in the adjacent skin led finally to the information that the woman had some 15 years before been hospitalized for pulmonary tuberculosis, for which she had received numerous pneumothorax refills over a period of 4 years. With each refill a fluoroscopy (or two) was generally performed, estimated to be some 200+ times in this particular woman. The total radiation dosage was guessed to be in excess of 4000 Rads to the breast area.

Thereafter a followup study was carried through on *877* female patients who had been hospitalized for the first time in 1940–49 for re-infection (adult) type tuberculosis. Of this group of patients *510* never received pneumothorax (*or* the accompanying fluoroscopies). *96* cases were started on pneumothorax, but because it was not operable this form of therapy was discontinued after a very brief trial, leaving *271* cases who received pneumothorax for extended periods, >6 months in most cases. With each refill, in general, a fluoroscopy (or two) was performed. The precise number of fluroscopies in each case could not be ascertained from the records. We shall return to this issue later.

These 877 patients were the "cohort" admitted during the decade 1940–1949, so that some 15–20 years elapsed between hospitalization and when Mackenzie's study was made. A diligent search of all the cases was made for the occurrence of carcinoma of the breast. Mackenzie found no evidence of bias, nor is it likely that the search was of any different degree of diligence for those who *had* pneumothorax as compared with those who had not received that therapy. The results, reproduced below, were startling and disturbing:

In 510 patients *without* pneumothorax therapy *one* breast cancer was discovered to have occurred subsequent to hospitalization.

In 271 patients with extensive periods of pneumothorax (and its concomitant fluoroscopies) there were 13 breast cancers in the follow-up period.

This is a ~*24* fold greater incidence of breast cancer in the pneumothorax series than in the other series, and as Mackenzie pointed out this result is of very high statistical significance. ($p=0.001$). That the difference in breast cancer incidence rate is enormous is very clear. It remained for Mackenzie to inquire as to explanation for the large observed differences, and in this he was most thorough. He considered the possibility that a less diligent search had been made for breast cancer in the group without pneumothorax, but concluded there was no reason to suspect the data on this basis. Furthermore, the breast cancer incidence rate in the pneumothorax group was far higher than that expected for comparable age groups in the population-at-large.

Mackenzie considered the possibility that tuberculosis per se might increase the breast cancer rate. He could find no evidence thereof. Even if there *were* some association of prior tuberculosis with breast

cancer 15 years later in life; one would have to stretch this to believe tuberculosis treated with pneumothorax is either peculiar or more severe and increases breast cancer some *20 times* more than other tuberculosis. This is indeed remote. To add to this remoteness, one would have to postulate that tuberculosis requiring pneumothorax predisposes *not* to the ordinary form of breast cancer but to an uncommon form (inner half of breast) precisely in the region of maximum irradiation associated with the fluoroscopies.

After giving due and proper consideration to these remote, unlikely explanations for the excess breast cancer in the pneumothorax-treated series, Mackenzie cautiously suggested that the radiation delivered during multiple fluoroscopies (100–300 times) might very well be the etiologic agent in the induction of the tremendous excess of breast cancers in these women. While Mackenzie's caution in proposing this explanation is in the highest tradition of the science of epidemiology, it would seem extremely remote indeed that any *other* etiology than radiation is even worthy of serious consideration. As we shall see below the peculiar location of the breast cancers in the irradiated women points very strongly to the fluoroscopic examinations as the basis for the excess breast cancer incidence.

DOSE RESPONSE RELATIONS IN RADIATION-INDUCED BREAST CANCER (MACKENZIE DATA)

Direct estimation of the dose in rads to the breast was not possible for any of these cases, since the only item of information available was the approximate number of fluoroscopies. For our purposes, a reasonable *range* of dosage can be estimated, certainly an estimate good enough to determine *within a few-fold* what the doubling dose for radiation-induced breast cancer might be.

Mackenzie ascertained the type of fluoroscopic equipment in use in the tuberculosis treatment centers of the relevant period (1940's) and the usual type of use. His estimate indicated that generally the examinations must have been carried through with a dose rate to the breast of between 22 and 55 rads/minute, depending on whether or not aluminum filtration had been used. His evidence suggested it might or might not have been used, so we shall retain the range, 22 to 55 Rads per minute. The physicians had been advised to avoid exceeding 10 second exposures, but he found it was likely that longer periods of exposure were not uncommon. Let us, since it is conservative and *minimizes* the radiation effect, assume each examination was 50% longer than recommended, i.e. 15 seconds. This would mean $\frac{1}{4} \times 22$ to $\frac{1}{4} \times 55$ as the dose in Rads per exam $= \sim 6$ to 14 Rads.

From the separate careful study of records of 40 breast cancer patients who had received pneumothorax, he estimated the following *minimum* distribution of fluoroscopic examinations.

Fluoroscopies:	*Number of cases*
Under 100	15
100 to 200	16
201 to 300	6
>300	3
Total	40

Let us assign 50 fluoroscopies for the <100 group, and 400 fluoroscopies for the >300 group, and use the midpoint of the other intervals and mean number of fluoroscopies for the group. Then we calculate:

$$\text{Average No. of Fluoroscopies} = \frac{(15)(50)+(16)(150)+(6)(250)+(3)(450)}{40}$$

$$= \frac{750+2400+1500+1350}{40} = \frac{6000}{40}$$

$$= \mathit{150} \text{ times per patient}$$

Therefore at 6 to 14 Rads per fluoroscopy, this means the average patient probably received 900 to 2100 Rads. If the true number of rads were lower than this the case against radiation is *even worse* than it will be calculated. The only hope for any mitigation of effect per rad in breast cancer induction would be for the above dose estimates to be low. It seems extremely unlikely that the dosage could have been more than a factor of 2 higher, or radiation dermatitis would have been common in these cases of breast cancer following repeated fluoroscopies. Such dermatitis was of low frequency. We shall remember this factor of 2 on the high side below.

Now since 13 cases of breast cancer occurred in 271 women, this corresponds to 24.5 cases per 510 women with pneumothorax therapy compared with *1* case per 510 women without pneumothorax therapy.

Excess 23.5 cases

$$\text{Therefore } \frac{23.5}{1.0} = 23.5 \text{ Doubling Doses of Radiation}$$

Our estimated range for mean dosage is 900 to 2100 rads for the overall group.

$$\text{At 900 Rads, we have } \frac{900}{23.5} = \mathit{38.3} \text{ Rads as Doubling Dose}$$

$$\text{At 2100 Rads, we have } \frac{2100}{23.5} = 89.4 \text{ Rads as Doubling Dose}$$

Let us allow for 2 more possibilities to "help" radiation. Suppose the average number of rads were *twice the high* estimate, namely 4200 Rads (an unlikely figure)

Then

$$\frac{4200}{23.5} = \mathit{178.7} \text{ Rads as Doubling Dose}$$

Let us further suppose our incidence figures, based upon 13 and 1 give rise to too high a number of doubling doses. Let us cut this in half; say 11.7 doubling doses instead of 23.5. Then for the *extreme* dose and *minimum* number of doublings,

$$\text{We have } \frac{4200}{11.7} = 359 \text{ Rads as Doubling Dose for Breast Cancer Induction.}$$

So the range is 38.3 Rads – 359 Rads as the doubling dose for radiation induced breast cancer—*using extreme limits* for estimates. Even the highest figure, 359 Rads, is a damning one for radiation inducing breast cancer. The other extreme figure, 38.3 Rads, well within the possibilities, is very frightening in its implications.

But, thus far, we have approached the problem in what may be regarded as a crude "overall" approach. There is a very important refinement, which makes the *true* doubling dose for radiation-induction of breast cancer *much* lower than any of the estimates above.

Let us recall that the first case that came to Mackenzie's attention showed her breast carcinoma in the inner half of the breast. In a series of 44 cases of breast cancer with prior history of pneumothorax, Mackenzie found 72.8% were either centrally located or were in the inner half of the breast. As Mackenzie points out, quoting Haagenson (3) the usual distribution of malignant breast tumors shows the *outer* half of the breast predominantly involved. The implication is, of course, obvious,—the fluoroscopic beam was far more likely to irradiate the inner half or central region of the breast, and hence the cancers developed there.

But let us now be quantitative on this point. From Ackerman and Regato, we have the following data for spontaneous mammary cancer in women (4):

	Percent of cases
Upper outer quadrant	47
Lower outer quadrant	7
Upper inner quadrant	14
Lower inner quadrant	2
Central (nipple area)	22

Therefore, spontaneous breast cancer is $(22+2+14)=38\%$ either in inner half of breast or centrally located, in contrast to the 72.8% for the pneumothorax cases. Now we are in a position to estimate doubling doses for radiation-induced cancer more meaningfully. If one is studying *induction* of a particular cancer in a particular location, the appropriate comparison of the *induced* disease is with the spontaneous occurrence *in that same location*—not elsewhere in the organ or in another organ. This is elementary, but essential.

Let us return to our input data:

24.5 cases per 510 women with pneumothorax therapy

1 case per 510 women without pneumothorax therapy.

Now let us calculate the data for (inner half + central) cancers

For the irradiated group $(0.728)(24.5)=17.8$ cases per 510 women.
For the unirradiated group $(0.38)(1.00)=0.38$ cases per 510 women.
Excess $=17.42$ cases per 510 women.

$$\text{Doubling doses}=\frac{17.42}{0.38}=45.8 \text{ doubling doses.}$$

At 900 Rads, we have $\frac{900}{45.8}=19.7$ Rads, doubling dose.

At 2100 Rads, we have $\frac{2100}{45.8}=45.9$ Rads, doubling dose.

At 1900 Rads, we have $\frac{4200}{\ }=91.8$ Rads, doubling dose.

So at our extremes of likely dosages, the estimated doubling dose for radiation-induction of breast cancer lies between 19.7 and 45.9 Rads. (91.8 Rads, for *very* extreme upper limit of dose).

If we wish to allow for errors of small numbers, let us make the rash assumption that we have only ⅓ as many doubling doses. This still corresponds to 59.1 Rads to 137.7 Rads as the range of extreme doubling doses.

Thus, at the outside, it is hard to conceive that the true doubling dose for breast cancer is higher than 140 Rads, with most of the evidence indicating it is far more likely to lie in the neighborhood of less than 50 Rads!

THE WANEBO (A.B.C.C.) BREAST CANCER DATA—RADIATION INDUCTION

The studies of Mackenzie just discussed are for women in Nova Scotia, Canada. We can now turn our attention to the same problem, radiation-induction of breast cancer 7500 miles away in Hiroshima and Nagasaki, Japan. Let us examine the data concerning doubling doses for radiation induction of breast cancer in this epidemiologically very different group of humans.

The best epidemiological material in the Wanebo study is for cases of breast cancer arising in 10,142 women who were examined at least once as part of the Adult Health Study Sample. Secondly, the analysis can be restricted to 20 cases where *dosage is known* and where only the *definite* cases are considered. Thirdly this group is ideal since all the breast cancers arose between 1958–1966, so that the series is not diluted unduly by cases likely still to be in the latent period of radiation-induction of breast cancer. Nevertheless, the true final incidence will undoubtedly be higher as more time elapses. Taken from Table 2 of Wanebo's paper are the data reproduced here as Table 1. (Reference 2)

TABLE I

Total dose (Rads)	Median dose (Rads)	Number of examined women	Definite cases of breast cancer (1958–66)
Not in City ATB [1]	0	2,458	2
0 to 9	4.5	3,082	3
10 to 39	25.0	1,282	4
40 to 89	65.0	857	2
90 to 199	145.0	802	4
200 plus	~300.0	841	5
Total			20

[1] ATB = at time of bombing.

Note: Dose unknown, 840. 2 cases left out of analysis because dose unknown.

Out of the total of 22 cases, 20 are from groups where the dosage is estimated; 2 are from groups where dosage could not be estimated. Obviously, analysis must be restricted to the 20 cases for whom dosage is known.

First, is there a significant increase in breast cancer in the irradiated persons?

The categories involving moderate or high radiation doses are as follows:

Exposure (mean dose in Rads)	Exposed	Cases of breast cancer
~300	841	5
145	802	4
65	857	2
25	1,262	4
Total	3,762	15

For the very low exposure or non-exposure categories, we have:

Exposure (mean dose in Rads)	Exposed	Cases of breast cancer
0 (not in city)	2,458	2
4.5	3,082	3
Total	5,540	5

The ratios of breast cancer incidence in the "irradiated" to "non-irradiated" group is

$$\frac{15/3762}{5/5540}=\frac{(5540)(15)}{(3762)(5)}=\frac{83100}{18810}=\mathit{4.4}\text{ fold}$$

There would seem no reason to doubt Wanebo and co-workers' conclusion that a *highly significant* association is noted in these data between radiation and subsequent appearance of breast cancer.

DOUBLING DOSE FOR BREAST CANCER INDUCTION BY RADIATION

To avoid dealing with small numbers and their statistical fluctuations, we shall make estimates of doubling doses only for combined groups with a reasonable number of cases.

(*a*) *All Cases where dose is over 90 Rads*

$$\text{Mean Dose}=\frac{(802)(145)+(841)(300)}{803+841}$$

$$=\frac{116290+252300}{1643}=\frac{368{,}590}{1643}$$

$$=224.3\text{ Rads}$$

In this over 90 Rad group, 9 cancers in 1643 women, or a rate of 54.8 cancers per 10000 women.

Now we need "spontaneous" rate of occurrence of cancer. We shall use the combined data of the Not-in-City group plus the 0–9 Rad group (assuming 4.5 Rads will hardly affect incidence *compared* with 224.3 Rads)*

Not in City=2458 women .... 2 breast cancers.
0–9 Rads=3082 women .... 3 breast cancers.
Total 5540 women .... 5 breast cancers.

So, "spontaneous" rate=9.0 cancers per 10000 women.

*A second-order correction could be made for this small effect here. One should not conclude 4.5 Rads is a negligible dose, however).

Excess Cancers=54.8 −45.8 per 10000 radiation-induced.

$$\frac{45.8}{9.0}=5.1\text{ Doubling Doses are represented.}$$

5.1 Doubling Doses=224.3 Rads
1 Doubling Dose = *44 Rads*

(*b*) *All Cases where Dose is over 40 Rads*

For the over 40 Rad group.

$$\text{Mean Dose}=\frac{(857)(65)+(802)(145)+(841)(300)}{857+802+841}$$

$$=\frac{55705+116290+252300}{2500}$$

$$=\frac{424295}{2500}=\mathit{169.7\ Rads}$$

In the over 40 Rad Group, 11 cancers in 2500 women, corresponding to a rate of 44 cancers per 10000 women.

"Spontaneous" rate (see above)=9.0 cancers per 10000 women

Excess Cancers=35.0 cancers per 10000 radiation-induced.

$$\frac{35.0}{9.0}=3.9\text{ Doubling Doses are represented}$$

3.9 Doubling Doses=169.7 Rads
1 Doubling Dose = *43.5 Rads*

(*c*) *All Cases where Dose is over 10 Rads*

For the over 10 Rad group.

$$\text{Mean Dose}=\frac{(1262)(25)+(857)(65)+(802)(145)+(841)(300)}{1262+857+802+841}$$

$$=\frac{31550+55705+116290+252300}{3762}=\frac{455845}{3762}$$

Mean Dose=121.2 Rads

In the over 10 Rad group, 15 cancers in 3762 women, corresponding to a rate of 39.9 cancers per 10000 women.

"Spontaneous" rate (see above)=9.0 cancers per 10000 women
Excess cancers=30.9 cancers per 10000 women, radiation induced

$$\frac{30.9}{9}=3.4\text{ Doubling Doses are represented.}$$

3.4 Doubling Doses=121.2 Rads

1 Doubling Dose=*35.6 Rads*

Now we can compare these results to see whether doubling dose is varying significantly as we include progressively lower dose categories. Linear theory would demand that the doubling dose remain constant in this test.

Group at risk:	*Doubling dose*
All cases above 90 Rads	44.0
All cases above 40 Rads	43.5
All cases above 10 Rads	35.6

Within the experimental error, linear theory is followed perfectly. If there is *any* deviation, the lowering of doubling dose as the lowest dose category is included suggests the risk of breast cancer per rad is more serious (at low dose) than is predicted by linear theory. This effect cannot be proved significant here, however. Above all, there is *no* suggestion of comfort in these data for the "threshold hopers". Recall, a threshold means that in the neighborhood of such a threshold, the doubling dose is trending to *infinity*. In the data above, doubling dose is trending *down*, if anything—*not* toward infinity.

Overall, the Wanebo data indicate that the doubling dose for radiation induction of breast cancer is in the neighborhood of 40 Rads.

But, the *true* doubling dose is probably somewhat lower than this. This must now be considered. Wanebo and co-workers indicate that the mean age of the patients with breast cancer A.T.B. who received 50 Rads or more is 28.1 years versus 39.8 years for those who received less than 50 Rads, or were not in the city A.T.B. Or, expressed as age at time of onset of breast cancer 43.3 years versus 55.3 years. So those who were more heavily irradiated developed breast cancer 11 or 12 years earlier than those who were not heavily irradiated. We don't have the cases split by radiation dose at 50 Rads, but we do above at 40 Rads. Since breast cancer *more than doubles* in 11 or 12 years spontaneously the appropriate comparison in the "spontaneous" groups would be the rate for women 11 or 12 years younger than the group we have. Let us be conservative and divide the incidence rate for the spontaneous group in half. We have then (from above)

In over 40 Rad Group, rate=44 cancers per 10000 women

Spontaneous rate=9/2=4.5 cancers per 10000 women

Excess cancer=39.5 cancers per 10000 women, radiation induced

39.5/4.5=8.8 Doubling Doses

8.8 Doubling Doses=169.7 Rads

$$\text{Therefore 1 Doubling Dose}=\frac{169.7}{8.8}=\textit{19.2 Rads}$$

This value is much more likely to be near the correct value than the 40 Rad region as the doubling dose for radiation-induction of breast cancer in the Japanese women.

From our analysis of the Mackenzie data, the best estimate of doubling dose lay between 19.7 Rads and 45.9 Rads.

The similarity of these doubling doses for such vastly different epidemiological population samples, receiving their irradiation in a different manner is indeed remarkable. Probably the data prove that humans are more alike than some humans think they are, at least with respect to susceptibility to radiation carcinogenesis.

CONCLUSIONS

(1) Analyses of Mackenzie's data on women developing breast cancer subsequent to irradiation by multiple fluoroscopies associated with pneumothorax therapy indicate a best estimate for the doubling dose as ~20 to 46 Rads for radiation-induction of breast cancer.

(2) Analysis of the very convincing data of Wanebo on Hiroshima-Nagasaki survivors indicates a best estimate of ~19.2 Rads as the doubling dose for radiation-induction of breast cancer.

(3) The agreement between the Canadian and the Japanese data is truly startling.

(4) The data indicate that linear theory holds up very well, in the entire dose region. (40–300 Rad region in Japanese and 900–2100 Rads in the Nova Scotia women). No suggestion of any sort that thresholds exist. If anything, the opposite is indicated.

(5) In our "laws" of carcinogenesis, we suggested previously a central value of ~100 Rads as doubling dose and a 1% increase in cancer risk per rad (5). As we indicated there, we were trying to be as conservative as possible, so as *not* to overestimate the hazard, but we stated there,

"Furthermore, we would estimate that the absolute numbers, if anything, probably underestimate the risk. For purposes of setting radiation tolerance guidelines, one might even be advised to use lower doubling doses than estimated above" (5).

The more we refine the calculations, the more it appears that this quotation is correct, and that the true doubling doses will be *lower* than we estimated. Certainly these persented here for breast cancer are definitely lower than 100 Rads. Of course, this would mean that our estimate of 16000 additional cancers per year from FRC Guideline dosages might be nearer 32,000, or even higher (5). We would like to check further doubling doses for other cancers in a refined manner, before taking this necessarily pessimistic position.

(6) It is puzzling to us to try to understand Storer's (6) and Miller's (7) difficulties in accepting the radiation-induction of human breast cancer, especially in view of the excellent agreement between the two vastly different epidemiological samples.

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Radiation-Induction of Human Lung Cancer

(By John W. Gofman and Arthur R. Tamplin)

[This is Document No. 7 in a Series of 7. Issued, Document 3, 4, 6.]

(a) THE HIROSHIMA-NAGASAKI DATA ON LUNG CANCER INDUCTION BY RADIATION

Wanebo and co-workers, utilizing several separate, but interdependent samples of the Hiroshima-Nagasaki Atom bomb survivors, reported a significant increase in lung cancer in those exposed to ionizing radiation (1). Summarizing their experiences they indcated:

(a) Based upon death certification in the large JNIH–ABCC* Life Span Study the observed to expected ratio for lung cancers was 1.9 times in those exposed to 90 or more rads.

(b) Based upon principal autopsy diagnoses, the observed to expected ratio of lung cancer was 2.15 times in those exposed to 90 rads or more.

(c) Based upon the ABCC–JNIH Adult Health Study the observed to expected ratio was 1.6 for those exposed to 90 rads or more.

Significance tests leave little reason to doubt the increase in lung cancer associated with radiation, as proposed, by Wanebo and co-workers.

Yet, Miller recently expressed doubt, and Storer has quoted Miller in these doubts—doubts we shall show here rest upon totally erroneous, indefensible scientific grounds. (2) (3).

Miller stated that the Japanese results are weakened by "the finding that the lung cancers were non-specific as to histologic type, rather than of the small cell type, as in U.S. uranium miners and in workers heavily exposed to mustard gas, a radiomimetic chemical."

Miller has, in our opinion, made a great error in his statement. This arises out of the data from the lung cancer story among the U.S. uranium miners. In those studies Saccomanno showed that small cell, undifferentiated cancers are 57% of all lung cancers in the uranium miners, whereas they are 20% or fewer of all lung cancers in non-miners (4). Out of these *important* observations, a mythology has arisen, obviously perpetuated by Miller, that radiation induces a *specific* form of lung cancer, namely that which is histologically of the small cell, undifferentiated variety.

In a previous report of this series, we have demonstrated that this is a grossly erroneous interpretation of the lung cancer findings in the uranium miners(5). The Saccomanno observations, properly utilized, demonstrate that:

*JNIH–ABCC=Japanese National Institutes of Health-Atom Bomb Casualty Commission.

(a) Radiation induces *both* bronchiogenic lung cancer *and* small cell, undifferentiated cancers of the lung.

(b) The reason for the preponderance of small cell cancers of the lung in the miners is that the domain of cells giving rise to such cancers receives *more* irradiation from the radon-daughters than the domain of cells giving rise to bronchiogenic cancers.

Therefore, since the basis for so-called *specific* lung cancer induced by radiation has been exploded, the grounds upon which Miller's criticism of the Japanese data rest simply no longer exist. We, therefore, believe it is appropriate to dismiss Miller's comments as not being justified in any way.

Storer's doubts rest largely upon Miller's comments concerning specificity of radiation-induced lung cancer, which is erroneous as shown above.

There does remain, with respect to the Japanese data, an effort to determine the doubling dose for lung cancer by ionizing radiation. This we shall address now. Unfortunately, not enough time has elapsed in the Japanese study so that the latency-period for lung cancer induction is fully over. Therefore, in studies completed by 1966, it is anticipated that the number of radiation-induced cases observed per year will be *less* than they would be when latency is fully over.

For purposes of conservatism, we shall underestimate the probable radiation effect, and assume that in the period of 1950–1966 (5 to 21 years after exposure), ⅔ of the rate of appearance of radiation-induced cancers would be observed compared with the rate to be observed at some later period when latency is over.

The most rigorously studied group of Wanebo et al are 66 lung cancers in Hiroshima and Nagasaki, observed to occur between 1950–1966. Ideally we would like to have a *larger* series starting 10 or more years after the bombing, but that must await further ABCC studies.

In Wanebo et al (Reference 1, Table 4) are presented the following data for 66 cases of lung cancer, male plus female, together with an "expected" incidence for each dose category, assuming no effect of radiation.

1950–66 (HIROSHIMA AND NAGASAKI)—TOTAL CASES LUNG CANCER, RADIATION-DOSAGE CATEGORY (RADS)

	200+	90–199	40–89	20–39	10–19	0–9	Unknown	Not in city
Observed	8	9	8	5	7	11	5	13
Expected	5.09	5.59	5.59	4.21	4.02	20.49	4.32	16.73

(5 cases out of 66 were in subjects whose dose was unknown)

We could do the analysis directly, but for convenience we shall estimate the population-at-risk for each dose category by what amounts to the inverse of the procedure used by Wanebo et al to arrive at the "expected" numbers of cases of lung cancer.

There are 66 total cancers occurring in ~15000 subjects (Wanebo states 15006 were examined in the first cycle). To arrive at "expected" numbers, what was done was to assume *no* effect of radiation, and then,

$$\frac{\text{population in a dose category}}{15000}=\frac{\text{Expected}}{66}$$

Transposing, we have

$$\text{Population in dose category}=\left(\frac{\text{Expected}}{66}\right)(15000)$$

In this way we arrive at the following approximate population-at-risk values for the various dose categories, and we are confident these must be extremely close to the true values.

Dose category (Rads)	*Population-at-risk (for 15,000 total)*
200+	1,148
90–199	1,270
40–89	1,270
20–39	957
10–19	914
0–9	4,657
Not in city	3,802
Unknown	982
Total	15,000

(Actually, if the total size of the Adult Health study sample is >15000, or <15000, the tabulated numbers are still totally usable, since all frequencies would be thereby be changed by a constant multiplier).

Now we can proceed to estimate doubling dose as follows:

I. *Mean Dose Calculation*

Above 40 Rads, we have

1148 subjects >200+ Rads (Use 300 Rads as median dose)
1270 subjects 90–199 Rads (Median dose=145 rads)
1270 subjects 40–89 Rads (Median dose=65 rads)

$$\text{Mean Dose}=\frac{(1148)(300)+(1270)(145)+(1270)(65)}{1148+1270+1270}$$

$$\text{Mean Dose}=\frac{344{,}400+184{,}150+82550}{3688}=\frac{611100}{3688}$$

Mean Dose=*165.7* rads

Occurrence of Cancers

In the irradiated group, we have 8+9+8=25 lung cancers in 3688 persons at risk, or a rate of 67.8 cases per 10,000.

For the spontaneous incidence we can use those not-in-city at time of bombing plus the 0–9 Rad group. (This gives an overall group with radiation exposure of ~ 5 Rads, which dose we can neglect *compared* with the mean of 165.7 Rads above).

So, "spontaneous" incidence=11+13=24 cancers in 4657+3802 persons, or 24 cancers in 8459 persons at risk. This corresponds to a rate of 28.4 per 10,000.

Excess cancers, radiation induced, =67.8−28.4=39.4 cases/10000 persons. Now, to correct for the fact that these observations were made with many subjects still in the latency period, we shall use the factor 3/2, described above.

Excess lung cancer, latency corrected, $3/2 \times 39.4 = 59.1$ cancers/10000.

$$\text{Doubling Doses} = \frac{\text{Excess}}{\text{Spontaneous}} = \frac{59.1}{28.4} = 2.08 \text{ doubling doses.}$$

But 165.7 Rads correspond to this excess,

$$\text{So, 1 Doubling Dose} = \frac{165.7}{2.08} = 79.7 \text{ Rads}$$

Note: Since we have been very conservative in correcting for latency in the 1950–1966 sample, our expectation is that the *true* doubling dose for radiation induction of lung cancer must lie below 79.7 rads. This value is highly consistent with all the other data we have previously presented, which indicated the doubling doses for human radiation carcinogenesis to lie in the neighborhood of 100 rads, with a high probability it may even be a factor of two lower (8) (9).

Certainly, as additional time elapses, new cases from Japan will allow further refinement of this doubling dose calculation. We can see no valid reason either for Miller's or Storer's doubts about the Japanese data. They appear quite firm, and are consistent with all other data concerning human radiation carcinogenesis.

(B) AN HYPOTHESIS THAT FLUOROSCOPIC RADIATION IS THE UNDERLYING CAUSE OF EXCESS LUNG CANCER IN TUBERCULOUS PERSONS

Recently, Steinitz, in Israel, published a paper probably destined to become a classic in the literature of epidemiology (10). It is entitled "Pulmonary Tuberculosis and Carcinoma of the Lung. A survey from Two Population-Based Registers". The conclusion of that paper is that patients with pulmonary tuberculosis histories have 5 to 10 fold the expected risk of later cancer of the lung—a risk increase comparable with that for heavy smoking of cigarettes.

We shall consider the Steinitz data and then show that

(a) The Steinitz observations are sound. The most probable quantitative explanation of the observed association of later lung cancer with tuberculosis therapy *is the fluoroscopic radiation* associated with collapse therapy of tuberculosis, such as pneumothorax.

(b) A world-wide study of the records of patients who have been hospitalized in the past for pulmonary tuberculosis should be made; particularly with respect to fluoroscopic radiation exposure and the subsequent development of lung cancer. The records are available *now;* it is a matter of studying them. If this is done, an epidemiological base would become available for the study of radiation-induced cancer of the chest region (lung and other) that is perhaps 100 times the size of the Hiroshima-Nagasaki exposed group.

(c) If our explanation for the Steinitz observations is correct, the need for drastic reduction in fluoroscopic exposure of tuberculosis patients will be evident, if they are to have their enormously excessive lung cancer risk lowered. Undoubtedly the recent lessened use of collapse therapy has already reduced fluoroscopic radiation exposure. To the extent that fluoroscopies are still used, and need to be used, modern techniques with dosage reduction become imperative as part of management of pulmonary tuberculosis patients.

THE STEINITZ OBSERVATIONS

Steinitz points out that in the very early literature, the general theme was expressed that pulmonary tuberculosis and lung cancer were almost mutually exclusive. Co-existence of the two diseases was, even up to recent decades, sufficiently rare, she notes, as to be worth literature reporting. Early it was considered that the rarity of co-existence of the two diseases was due to the fact that tuberculosis killed so many patients early in life that they didn't reach the age where bronchiogenic cancer of the lung became prominent as a general cause of death. But as Steinitz describes, many more recent reports have noted a high frequency of lung cancer and tuberculosis in the same person, and controversy has recently existed concerning tuberculosis as *an* etiologic contributor to lung cancer.

Steinitz was prompted to carry out her epidemiologic study for the following interesting reason. "Some two years after starting a cancer registry in 1960, Israel noted that case records of male patients with pulmonary carcinoma contained tuberculosis *approximately seven times* as frequently as those of stomach cancer in the same age groups." The suspicion of an association of *lung* cancer with prior tuberculosis was therefore high, and led to a detailed epidemiologic study of essentially the entire Israeli population. As we shall see later an association of lunch cancer with prior tuberculosis and a *failure* of such association for gastric cancer is *precisely* what would be expected if fluoroscopic radiation had caused the excess lung cancers in persons with prior tuberculosis.

Two separate studies were conducted by Steinitz

(a) A comparison of malignant neoplasms, including pulmonary neoplasms in the total Israeli population and in the population under supervision for prior tuberculosis.

(b) The autopsy data on 1155 cases of carcinoma of the lung were reviewed with respect to presence or absence of a history of tuberculosis.

Both studies were completely consistent with each other, indicating a 5 to 10 fold higher risk of lung cancer in persons who had had tuberculosis serious enough to require hospitalization therapy at some past period.

The Risk of Lung Cancer in Persons With Tuberculosis

Several important inputs were required for the Steinitz data:

(a) The distribution of pulmonary tuberculosis cases in the Israeli population.

(b) The distribution of heavy smoking in the Israeli population.

(c) The distribution of new primary lung cancer cases (1960–63).

(a) The distribution of pulmonary tuberculosis cases in Israel

Two independent sources of information were available to Steinitz

(a) the tuberculosis registry.

(b) the results of mass radiographic examination of 500,000 Israelis between 1952 and 1962.

Both led to consistent results. The prevalence data from mass radiography are reproduced here (Table 6 Reference *10*).

STEINITZ DATA—PREVALENCE OF PULMONARY TUBERCULOSIS (ALL GRADES OF DISEASE ACTIVITY) IN THE JEWISH POPULATION, BY AGE AND SEX (BASED UPON MASS RADIOGRAPHY 1952–62)

	Number of examinations	Prevalence of pulmonary tuberculosis rate per 1,000 examined
Males:		
All ages	291,002	11.63
45 to 64 years	51,212	37.18
65 and over	10,211	40.83
Females:		
All ages	231,336	8.55
45 and over	44,087	21.15

From these data, it is readily calculated that, for example, for men 45–64 years of age, 37.18 per 1000

$$\text{So for 51212 persons, tuberculosis} = \frac{51212}{1000} \times 37.18 = 1904.1$$

$$\text{Therefore, \% of such males with tuberculosis} = \frac{1904.1}{51212} \times 100 = 3.72\%$$

In a similar manner the % prevalence of tuberculosis can be calculated for each age group.

From independent data, Steinitz estimated the % prevalence of heavy cigarette smokers (20 or more/day) in Israel.

New cases of primary lung cancer for the period 1960–63 in Israel were 837 distributed between males and females and into the different categories of age, sex, tuberculosis history, and cigarette smoking. A very few cases were *both* heavy smokers and tuberculosis. 9 such occurred in the men, 45–64 years of age, and 4 such occurred in the men over 65 years of age. Steinitz did the calculations of risk of lung cancer (see below) with these cases included and then excluded for both the smoking and the tuberculosis categories.

The results including risks of pulmonary carcinoma are reproduced from Steinitz (Table 7, Reference *10*)

The results are striking, and significant.

Males (45–64 years), (with tuberculosis of all degrees of activity), show a rate of development of primary lung cancer *7.98* to *9.76* times as high as non-tuberculous persons. Indeed the risk of lung cancer, if tuberculous, is comparable to the risk for heavy cigarette smokers.

Females (45 or over) show a 10.8 fold higher rate of development of primary lung cancer, if tuberculous, when compared with the rest of the population.

Males (65 years or older) shows a 5.34–6.28 fold higher rate of development of primary lung cancer, if tuberculous, then do all others. Again, the risk of lung cancer, if tuberculous, was comparable to the risk for heavy smokers of cigarettes.

In a similar study, based upon review of autopsy data, Steinitz demonstrated that malignant neoplasm is excessive as a cause of death in patients with pulmonary tuberculosis *and* that the *excess* malignancy is totally accountable as *pulmonary* malignancy. She quotes Campbell (11) and Gebel (12) as having found similar results.

TABLE 7 REFERENCE 10

	Distribution of population according to prevalence: Percent	Number	New cases of lung cancer: Number	Rate per 100,000	Period 1960–63 risk relative to general population
MALES					
Age 45 to 64	100.00	207,424	252	518–634	7.98–9.76
Tuberculous persons	3.72	7,722	[1]49 +9	518–634	7.98–9.76
Heavy smokers	8.72	17,928	94	474–524	7.30–8.07
Other	87.56	181,774	118	64.9	1.00
Age 65 and over	100.00	55,179	160		
Tuberculous persons	4.08	2,251	[1]27 +4	1,020–1,199	5.34–6.28
Heavy smokers	6.00	2,210	42	1,148–1,269	6.01–6.64
Other	89.92	49,618	95	191	1.00
FEMALES					
Age 45 and over	100.0	257,200	83		
Tuberculous persons	2.12	5,453	15	275	10.8
Heavy smokers	1.97	5,067	5	98	3.84
Other	95.91	246,680	63	25.5	1.00

[1] Cases both reported as heavy smokers and as tuberculous. Rates calculated with and without these cases in each group. Maximum rate includes these cases. Minimum rate excludes these cases. This is the reason for the ranges given.

Note: Estimate of risk of developing primary carcinoma of the lung (ICD code No. 162) for patients with pulmonary Tuberculosis and for heavy smokers, compared with that for the General population (Jews) by age and sex.

Fluoroscopic Radiation as the Basis for the Excessive Rate of Occurrence of Pulmonary Cancer in Persons with Tuberculosis

Steinitz' data show some 5–10 fold higher rate of primary lung cancer in Israelis with pulmonary tuberculosis, active or otherwise. Her earlier studies indicated no such excess for gastric cancer in persons with a history of tuberculosis.

How are these results to be explained? One suggestion has been that the presence of pulmonary tuberculosis causes changes in lung or bronchial cells that might predispose to carcinoma. It can be pointed out that from other studies, the site of carcinoma is often grossly different from the site of tuberculosis. This argues against the above explanation (13). But there are far more potent arguments.

We shall, for purposes of argument, reject totally that lung changes due to tuberculosis are responsible for a high risk of subsequent lung cancer.

Instead, we *propose* that *fluoroscopic radiation* in the course of tuberculosis therapy is *the etiologic factor* responsible for the excessive lung cancer in such persons.

Why do we propose this hypothesis?

In a previous report of this series we analyzed the Mackenzie study of breast cancer in women with tuberculosis (14). His data showed that women with prior tuberculosis treated by pneumothorax collapse therapy had a 24 fold higher incidence of subsequent breast cancer than did women in the same sanitorium with tuberculosis but *without* such pneumothorax therapy.

From Mackenzie's data we estimated that the ~150 fluoroscopies associated with pneumothorax probably resulted in a delivery of between 900–2100 rads to the chest for the women treated with pneumothorax.

Now, the Mackenzie data are for a sanitorium in Nova Scotia. One cannot prove that the Israelis, wherever they were when treated for tuberculosis, would have received precisely the same fluoroscopic radiation dose. On the other hand, for lung cancer showing up in 1960–63 (Steinitz data) and a 15 year latent period, the Israelis treated most likely received therapy comparable to other areas in the 1940's and early 1950's. Pneumonthorax was widely practiced then, with its accompanying numerous fluoroscopies. Since for present purposes, only an approximation is needed, we shall consider the *expectations* anywhere in the world if tuberculosis therapy were like that of the sanitorium reported by Mackenzie.

He had

877 total patients treated
271 received prolonged pneumothorax
607 did not

Let us calculate average dose to *all* patients, realizing that only the pneumothorax cases received appreciable irradiation, using our two estimated limits, 900 Rads and 2100 Rads.

At 900 Rads Mean Dose=

$$\frac{(271)(900)+(607)(0)}{877}=\frac{243900+0}{877}=278.1 \text{ Rads}$$

At 2100 Rads Mean Dose=

$$\frac{(271)(2100)+(607)(0)}{877}=\frac{569100}{877}=648.9 \text{ Rads}$$

Let us use, for argument, the central value between these two values, or *464* rads for the average patient in Mackenzie's sanitorium group. Now let us consider the implications in *any* sanitorium where similar practices prevailed.

We have shown earlier in this report that <80 Rads is a reasonable value for the doubling dose for lung cancer in the Japanese data. From other studies, we shall not be surprised if the final value turns out to be 50 rads.

Let us explore 100 Rads as doubling dose:

For a group receiving 464 Rads, this is 4.64 doubling doses. Therefore the total rate for future expected lung cancer is

1.0 for the spontaneous incidence
4.64 for the radiation induced excess

5.64 is the *factor* of total increase in lung cancer expected.

Let us explore 50 Rads as doubling dose for lung cancer

$$\frac{464}{50}=9.3 \text{ doubling doses}$$

Therefore the total rate of future expected lung cancers is

1.0 for the spontaneous incidence
9.3 for the radiation-induced excess

10.3 is the *factor* of total increase in lung cancer expected.

This range of 5.6 to 10.3 fold increase in expected lung cancer rate is so similar to the observed increase in Israel as to lead us to one conclusion—Most probably the radiation dosage in fluoroscopies associated with tuberculosis therapy in the Israel group was probably closely similar to that in Mackenzie's Nova Scotia cases.

We suggest that if Steinitz investigated the fluoroscopy-pneumothorax incidence in her cases, she will, in all probability, confirm our suspicions of radiation-induction of her excess lung cancers, qualitatively and quantitatively.

The much lesser, if any, excess of gastric cancers in tuberculosis subjects is precisely as expected—if the X-rays don't strike an organ, no excess cancer occurs.

(C) A PROPOSAL FOR A 100 FOLD INCREASE IN IMMEDIATELY AVAILABLE INFORMATION CONCERNING RADIATION-INDUCTION OF LUNG CANCERS

Only very gradually do the new cases add to the lung cancer statistics in Hiroshima-Nagasaki. Through the vistas opened by Steinitz' remarkable epidemiologic study and our analyses here presented, there are immediately available at least 100 times as many lung cancers for analysis, and possibly several thousand times as many.

World-wide, in well-run tuberculosis hospitals where records *are* kept, there must have been millions of patients treated for tuberculosis, say between 1930–1950, during which time pneumothorax collapse therapy, with its associated fluoroscopies, was widely practised (as it was in Nova Scotia). If a study is made of the records in such hospitals, together with follow-up data on the occurrence of primary lung cancer, there should be many thousands of lung cancers available for analysis. It must be emphasized that for persons hospitalized in 1930–1950, the latency period for radiation-induced cancers is past, so that cases *must already* have occurred—and it is simply a matter of record search to ascertain how many cases occurred in each category of fluoroscopic exposure. As Steinitz pointed out, a fraction of the lung cancers will be lost because it is all too easy to assume that pulmonary disease in persons with a prior history of tuberculosis *is* tuberculosis. *But* this should be an equal loss for previously fluoroscoped persons and those not receiving radiation, and hence should not affect the outcome.

We urge the Epidemiology Section of National Cancer Institute, the WHO, and every tuberculosis sanitorium in the world to do this record study. Properly done, it will resolve the lung cancer radiation-induction problem, including the *overall* dose response curve far earlier and far better than waiting for the ABCC studies to mature further.

Additionally, we predict the following if such a study is done—

(a) Leukemia will be found to be increased in the fluoroscopically irradiated tuberculous subjects.

(b) Mediastinal tumors, e.g. lymphomas, will also be found increased-separate from lung cancers.

(c) Both bronchogenic *and* small cell undifferentiated cancers will be found increased.

(d) Possibly even bone sarcomas will be shown increased, if the studies are pooled.

(e) If other sites of cancer are checked at the same time in these studies, the further the organ from the chest, the lower will be any evidence of radiation induction of cancer.

Such studies may well already be underway. If so, we urge their expansion broadly!

(D) SOME IMPORTANT SUGGESTIONS FOR DECREASING THE HIGH RISK OF LUNG CANCER IN PERSONS WITH TUBERCULOSIS

The 5 to 10 fold higher rate of pulmonary cancer found by Steinitz in persons with a record of tuberculosis is shocking and terribly important. We have presented evidence to make us believe these cancers were provoked by fluoroscopic irradiation. Confirmation should not be long delayed, since only record search is required.

In recent years, pneumothorax with its associated fluoroscopies has waned greatly as a form of tuberculosis therapy. This is excellent, for radiation-induced lung cancers should decrease as a result.

However, fluoroscopy with or without pneumothorax, is undoubtedly still a highly frequent procedure in the management of pulmonary tuberculosis. Obviously where the best therapy of the patient with tuberculosis requires fluoroscopies, they should be done. But, as emphasized by Morgan, techniques are available for grossly reducing the radiation-exposure with such fluoroscopies (15). Every effort to achieve this must be carried through in the treatment of tuberculosis, if we are right.

Steinitz, expressing her horror of the lung cancer risk in the subjects with tuberculosis, pleads for more effort to *prevent* tuberculosis. Of course we agree with Steinitz that prevention of tuberculosis is good. But we suspect the real horror comes from the radiation in the course of therapy, rather than from the tuberculosis per se.

Steinitz deserves the world's appreciation for her monumental epidemiological contribution to a vital subject.

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The Mechanism of Radiation Carcinogenesis

(By John W. Gofman and Arthur R. Tamplin)

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INTRODUCTION

All responsible bodies concerned in setting standards for radiation of humans properly discount the possibility that fractionation of the radiation over time will mitigate carcinogenic effects of ionizing radiation (1). Nevertheless, everyone, ourselves included, has always cherished the fond hope that perhaps fractionation of radiation might in some way mitigate the carcinogenic risk.

There are those who speak of "repair"—meaning that in some way, not known, cells will repair damage that is carcinogenic, provided enough time elapses between successive radiation insults. No logic ever was presented that was particularly satisfying for what the mechanism of such "repair" might be, but since "hope springs eternal," we, for a long time, participated in such hopes—especially since the outlook is *so* unfavorable without this hope.

However, our recent researches have led us to examine the experimental animal data which underly such hopes for fractionation protection, and as a result of careful analysis, we feel that all such hopes are essentially without foundation, and extremely unlikely ever to materialize.

Recently, Dr. John Totter, specifically (2), and AEC–DBM in "Comments" (3) on our work have both indicated that we have largely ignored experimental animal data which suggest a lesser carcinogenic risk of fractionated radiation. While it should be emphasized that their comments should be totally irrelevant in the field of setting standards (witness ICRP approach), we do wish to comment on the totally erroneous AEC–DBM statements. We have certainly *not* "largely ignored" the experimental animal data. Indeed we have, for a long period, been studying such data and had planned to write in extenso why we don't believe they provide any hope whatever for fractionation in the mitigation of radiation carcinogenesis. Certainly, as any responsible public health official knows, such experimental data should not be part of the proper conservatism expected in the consideration of standards. This is why we omitted such considerations in our IEEE Paper and in the Senate Subcommittee Hearings (4) (5).

However, by now our researches are far enough along to allow us to make a presentation of why we consider the experimental data concerning fractionation protection to be an *illusion*, not a reality. Further, the presentation will allow us to present an integrated concept, including differences between protracted and acute low LET radiation, high LET versus low LET radiation, and the real nature of RBE (=relative biological effectiveness) for carcinogenesis.

The results lead us to be even more pessimistic concerning the expected number of additional cancers per year from FRC Guideline exposure than we previously were.

A PROPOSED MECHANISM FOR RADIATION CARCINOGENESIS

If the age-specific mortality rates in humans, for a particular cancer, are examined, it is noted that there is a rising age-specific mortality with age, and that for many cancers, there is a period of life where age-specific mortalities double over a five-year period. At some phases of life, and for certain cancers, the period may be less than five years or more than five years for this age-specific mortality doubling. For certain malignancies, e.g. leukemia, the age-specific mortality curve is complicated by an early peak, a decline with age, and then a steep rise.

While, for this analysis, we could have used any other period for the rate to double, we shall choose, as a reference, five years as the time to double the *spontaneous* incidence of a particular type of human cancer. No conclusions we draw will be materially altered if 6, 7, or 8 years were to be used as the doubling time. *This* concept of doubling time is hardly controversial, since it is just obvious from Vital Statistics (6).

Next, for a specific form of cancer that has been proved to be radiation-induced, there exists a dose of radiation that produces an excess of that cancer equal to its spontaneous rate of occurrence. This dose we have referred to as a *doubling dose* of radiation for that particular cancer. Furthermore, our analysis of data for specific cancers indicates that the doubling dose doesn't vary appreciably over a wide interval of total doses (7) (8) (9). This is what we mean when we use "linear" theory. Indeed, everything we have to demonstrate below does not require *absolute* constancy of doubling dose over all dosage ranges.

Previously we suggested that this doubling dose is approximately 5 rads in-utero or in early infancy, and increases to ~ 100 rads in adult life (5). We had suggested that considerable evidence suggested the adult doubling dose might be much lower than 100 rads.

Most of our recent examinations of data suggest 50 rads to be closer than 100 rads for the doubling dose for human cancers in adult life. We shall use 50 rads as the doubling dose here. Other values could be chosen without alteration of the principles to be presented below—the essential point is that the doubling dose *increases* as we go from early infancy to adulthood.

(a) *Translation of doubling dose to "effective aging"*

If 5 years of adult life doubles the age-specific mortality for a particular cancer, and if, separately, *50 rads* of ionizing radiation doubles that mortality similarly, it has been widely suggested, especially by Jones (10), that the radiation dose can be translated into a specified number of years of "effective aging", *at least* for carcinogenesis.

We shall make this translation, and state, for adults at 30 years of age,

5 adult years is equivalent to 50 rads, or
1 rad=0.1 adult year.

At the beginning of infancy, 5 rads appears to double the *future* incidence of cancer, so we can say 5 rads, in the milieu of the first year of

[illegible] adult years of effective aging with respect to cancer production, or

in early infancy, *1 rad=1.0 adult year.*

In the intervening years, between 0 years of age and 30 years of age, the milieu changes so that 1 rad drops from 1.0 adult year finally to 0.1 adult year. We shall explore more than one shape of curve for this transition. The shape of this curve is not crucial for the general principles, although, as will become obvious, it is important for absolute values.

(*b*) *Low LET Radiation* (*x-rays*, γ*-rays*, β*particles*) *and the Illusory Protection Against Cancer by Protraction of Radiation*

Upton is a leader in this general field of investigation. Recently, he pointed out the following: "In general, irradiation at a high dose rate is more effective than irradiation at a low dose rate, at least in the case of radiation of low linear energy transfer (LET), such as x-rays and gamma rays. When fractionated into several exposures of intermediate size and periodicity, however, a dose may be more tumorigenic than when given in a single brief exposure". (11)

DBM-AEC conveniently overlooks this last sentence when they herald the protection from fractionation. But to be as optimistic as possible, we will join DBM in overlooking that sentence, and focus on the "hopeful" side of the quotation—namely, that for low LET radiation a high dose rate is more carcinogenic than a low dose rate.

From what types of experiments does this hopeful note emerge? Upton has provided two representative experiments of the hopeful variety; (a) one for myeloid leukemia induction in RF male mice by Co^{60} gamma rays delivered chronically (0.0006 rads/min.) versus a single acute exposure of x-rays at 50–100 rads/minute delivered at 8–10 weeks of age; (b) a similar experiment for ovarian carcinogenesis on 10-week old RF female mice for γ rays at 7 rads per minute versus 5 rads per day.

Both experiments show that the final incidence of the particular leukemia, or ovarian cancer, is lower for the protracted radiation than for the acute radiation. Many people interpret such experiments to mean "repair" with respect to carcinogenesis is occurring in the protracted radiation. There is not the slightest scientific evidence for any such "repair", and indeed there is a more plausible explanation—an explanation that not only interprets these findings, but also *predicts the difference* between low LET and high LET radiation carcinogenesis. We shall provide this explanation below, but first we must examine the parameters of the experimental animal situation and translate them, as best as possible, to the *relevant* human situation. Let us consider the RF male mouse myeloid leukemia study.

APPROXIMATE COMPARISONS

	Mouse (RF male mice)	Man
Lifespan	~700 days	~70 years.
Acute dosage delivered	~60–70 days of life (1/10 lifespan)	~7–8 years of age (~1/10 lifespan).
Chronic dosage	0.0006 rads/min=0.86 rads/day or 0.86 rads per 1/700 lifespan.	~0.86 rads for 1/10 year or ~8–10 rads/year.

Now, if we were to do a comparable experiment in humans, would a lesser carcinogenic effect be observed by protraction? Would this argue

An explanation for the apparent effect of protraction [illegible] in reducing carcinogenesis for low LET radiation

We shall use comparable parameters to those of Upton, and study in man:

(*a*) Response to 0–100 rads of Low LET radiation
(*b*) Acute dose to be delivered at 8 years of age
(*c*) Chronic dose to be delivered at doses of 10 rads per year or even at *lower* dose rates, starting at 8 years of age.
(*d*) Calculate the comparative expectancy of cancer later in life.
(Similar in general to the Upton type of experiment).

We shall use our translation of rads to "effective aging" in adult years and basic physics.

Low LET radiation interaction with matter

At 1 MEV or less x-rays and γ-rays deliver energy to tissue through their photo-electric or Compton conversion to electrons, so we can cover this group of radiations by consideration of β-particles.

$$1 \text{ rad} = 100 \text{ ergs/gram} = 6.25\times10^{7} \text{ MEV/gram.}$$

For 1 MEV β-particles, this means 6.25×10^{7} β-particles per gram.

The range in tissue for 1 MEV β-particles is ~4000 microns. For a large cell (20 micron-diameter), 1 MEV β-particles traverses 200 cells, on the average.

Therefore, 6.25×10^{7} β-particles traverse 1.25×10^{10} cells.

For cells of ~20 microns, Volume $\cong 4\times10^{3}\mu^{3}$, so there are

$$\frac{10^{12}}{4\times10^{3}} = 2.5\times10^{8} \text{ cells per gram of tissue cells.}$$

Obviously, this means each cell is traversed much more than once

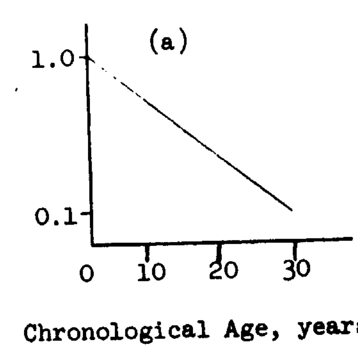
$$\text{for delivery of 1 rad } \left(\frac{1.25\times10^{10}}{2.5\times10^{8}}\right) \cong 50 \text{ times}\Big).$$

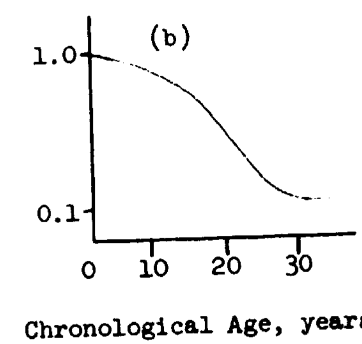
So for β-particle irradiation, we can say that one rad will provide approximately uniform irradiation of the cells. (As we shall see later, this is not so for high LET radiation, such as α-particles).

$$\text{Now previously we set} \begin{cases} \text{at age 30 years} & 1 \text{ rad} = 0.1 \text{ year of aging} \\ \text{at birth} & 1 \text{ rad} = 1.0 \text{ year of aging} \end{cases}$$

Change rate in the intervening period, we do not know, so we shall consider two possibilities, shown in the figure below.

Figure 1





[In years]

Tabular values for 1 rad (effective years of aging) curve (a)		Tabular values for 1 rad (effective years of aging) curve (b)	
Age	Value per rad	Age	Value per rad
0	1.0	0	1.0
2	.62	2	.77
4	.46	4	.64
6	.36	6	.55
8	.29	8	.46
10	.25	10	.30
12	.22	12	.205
14	.19	14	.15
16	.17	16	.125
18	.156	18	.11
20	.143	20	.106
22	.132	22	.103
24	.122	24	.101
26	.114	26	.100
28	.106	28	.100
30	.100	30	.100
>30	.100	>30	.100

Now let us compare acute and chronic radiation with low LET radiation, using first Curve (A) and then Curve (B) for the variation in value per rad at the various chronological ages.

Curve (A) calculations—10 rads total dose, acute delivery at 8 years of age

At age 8 years, 1 rad=0.29 years of "Effective Aging"; Therefore, 10 rads=2.9 years of "Effective Aging"

CHRONIC DELIVERY OF 10 RADS, AT 1 RAD PER YEAR FOR 10 YEARS, STARTING AT AGE 8

[Value per rad]

1 rad in 8th year	0.29
1 rad in 9th year	.27
1 rad in 10th year	.25
1 rad in 11th year	.23
1 rad in 12th year	.22
1 rad in 13th year	.21
1 rad in 14th year	.19
1 rad in 15th year	.18
1 rad in 16th year	.17
1 rad in 17th year	.16
Total (10 rads over 10-year period)	2.17

[1] Years effective aging.

In an entirely analogous manner we can calculate the "effective years of aging" for 20 rads, acute and chronic, 30 rads, acute and chronic, etc. etc. out to any dose we choose. Presented in Table 1 are the results out to 100 rads.

TABLE 1.—ACCUMULATED EFFECTIVE YEARS OF AGING BY ACUTE VERSUS PROTRACTED RADIATION (LOW LET) DELIVERED AT 8TH YEAR (HUMAN) ACUTE VERSUS OVER A 10-YEAR INTERVAL [1]

Total dose accumulated (low LET) (rads)	Cumulative effective years of aging: Acute exposure (8th year)	Cumulative effective years of aging: Protracted (delivered between 8th to 18th years)	Total dose accumulated (low LET) (rads)	Cumulative effective years of aging: Acute exposure (8th year)	Cumulative effective years of aging: Protracted (delivered between 8th to 18th years)
10	2.9	2.17	60	17.4	13.02
20	5.8	4.34	70	20.3	15.19
30	8.7	6.51	80	23.2	17.36
40	11.6	8.68	90	26.1	19.53
50	14.5	10.85	100	29.0	21.70

[1] Data from fig. 1a.

Now, in the usual type of experiment of this sort, one examines either the total accumulated cancers out to death, or the cancer occurrence rate at some later period in life. Both are valuable, but the major features of the story can be discerned by looking at age-specific mortality rate at, say, 40 years of age, chronological.

To determine the cancer mortality, we shall use the age-specific mortalities for all malignant neoplasms combined, derived from U.S. Vital Statistics for 1966 (USA) (Males) (6). Any specific malignant neoplasm could be studied, and indeed the results obtained will depend upon the age-specific mortality data for *that* neoplasm.

Reproduced in Table 2 are the Age-Specific mortalities for *all malignant* neoplasms by age.

TABLE 2.—AGE-SPECIFIC MORTALITIES, MALES, ALL MALIGNANT NEOPLASMS (BASED UPON U.S. 1966 VITAL STATISTICS)

Chronological age (Years)	Age-specific mortality (all malignant neoplasms cases per 10^6 persons per year)	Chronological age (Years)	Age-specific mortality (all malignant neoplasms cases per 10^6 persons per year)
Under 1	52.4	45 to 49	1,262.0
1 to 4	88.2	50 to 54	2,382.5
5 to 9	78.1	55 to 59	3,918.4
10 to 14	68.2	60 to 64	5,980.6
15 to 19	91.6	65 to 69	8,712.5
20 to 24	113.8	70 to 74	11,365.8
25 to 29	141.3	75 to 79	13,568.4
30 to 34	215.0	80 to 84	15,723.7
35 to 39	368.0	85 and above	18,123.2
40 to 44	691.4		

Now we can tabulate, for chronological age 40 years, both the effective ages (chronologic + radiation aging calculated above) for the acute and chronic irradiation, and the mortalities from all malignant neoplasms (from Table 2) for both groups.

TABLE 3.—CANCER MORTALITY IN RELATION TO ACUTE VERSUS PROTRACTED RADIATION

[40 years chronological age]

Rads	Acute radiation: "Effective age" (years)	Acute radiation: Cancer mortality (per 10^6/yr.)	Protracted radiation: "Effective age" (years)	Protracted radiation: Cancer mortality (per 10^6/yr.)
0	40.0	550	40.0	550
10	42.9	800	42.17	710
20	45.8	1,130	44.34	970
30	48.7	1,650	46.51	1,200
40	51.6	2,320	48.68	1,650
50	54.5	3,150	50.85	2,140
60	57.4	4,100	53.02	2,700
70	60.3	5,400	55.19	3,250
80	63.2	6,600	57.36	4,060
90	66.1	8,170	59.63	5,050
100	69.0	9,750	61.70	5,750

Clearly, there are many more cancers in the acute irradiation group than the chronic irradiation group, BUT it all derives from the fact

that the chronic irradiation was delivered at later and later periods of life when the *effectiveness per rad* is decreasing. So, instead of invoking mysterious, unknown "repair" mechanisms, we have an obvious explanation based upon *known* phenomena. Namely, irradiation early in life is much more serious in increasing cancer occurrence than irradiation later in life. These data are even more striking when presented as the excess, radiation-induced cancers in the next table.

TABLE 4.—RADIATION INDUCED (EXCESS) CANCERS FOR ACUTE VS PROTRACTED RADIATION (AT CHRONOLOGICAL AGE 40 YEARS)

Dose (rads)	Acute radiation excess cancers 10^6/years	Chronic radiation excess cancers 10^6/years	Ratio acute/chronic
0	0	0	
10	250	160	1.56
20	580	420	1.38
30	1,100	650	1.69
40	1,770	1,100	1.61
50	2,600	1,590	1.64
60	3,550	2,150	1.65
70	4,850	2,700	1.80
80	6,050	3,510	1.72
90	7,620	4,500	1.69
100	9,200	5,200	1.77

The factor of difference between acute and chronic exposure depends in part upon what age the mortalities are calculated for (40 years here), and the factor will be highly sensitive to the shape of the curve relating doubling dose at birth and at 30 years of age. These curves were demonstrated as Figure 1 (a) and (b).

Let us now recalculate the story using Figure 1(b) for the value of 1 rad in "effective years of aging" from birth out to 30 years.

TABLE 5.—ACCUMULATED EFFECTIVE YEARS OF AGING BY ACUTE VERSUS PROTRACTED RADIATION (LOW LET), DELIVERED AT 8TH YEAR (HUMAN) ACUTE VERSUS OVER A 10-YEAR INTERVAL

[Data from fig. 1b]

Total dose accumulated (low LET)	Cumulative effective years of aging: Acute exposure (8th year)	Cumulative effective years of aging: Protracted (delivered between 8th and 18th years)
0	0	0
10	4.6	2.32
20	9.2	4.64
30	13.8	6.96
40	18.4	9.28
50	23.0	11.60
60	27.6	13.92
70	32.2	16.24
80	36.8	18.56
90	41.4	20.88
100	46.0	23.20

Now we can calculate the effective ages and the corresponding "all malignant neoplasm" mortalities as before. These are presented in table 6.

TABLE 6.—CANCER MORTALITY IN RELATION TO ACUTE VERSUS PROTRACTED RADIATION

[40 years chronological age]

Rads	Acute radiation: "Effective age" (years)	Acute radiation: Cancer mortality (per 10^6/yr)	Protracted radiation: "Effective age" (years)	Protracted radiation: Cancer mortality (per 10^6/yr)
0	40.0	550	40.0	550
10	44.6	1,000	42.32	720
20	49.2	1,760	44.64	1,000
30	53.8	2,950	46.96	1,250
40	58.4	4,550	49.28	1,780
50	63.0	6,500	51.60	2,320
60	67.6	9,000	53.92	2,980
70	72.2	11,450	56.24	3,720
80	76.8	13,470	58.56	4,600
90	81.4	~15,700	60.88	6,350
100	86.0	~16,000-17,000	63.20	6,600

It can be noted here, for 50 rads, the ratio

$$\left(\frac{\text{acute, excess}}{\text{chronic, excess}}\right)=\frac{5950}{1770}=3.36$$

in contrast with 1.64 for the data derived from Figure 1a. Thus, *how* the doubling dose curve goes up from birth to 40 years is highly determinative of the *illusory* protection afforded by protraction.

We would like to compare our human curve more closely with Upton's mouse curve, but to do so requires knowledge of the precise nature of the doubling dose variation with age, and also would require the age-specific incidence of myeloid leukemia at specified periods in life for the mouse, which is not provided[11]. The general features are clearly similar in making it *appear* that protraction lessens carcinogenesis. In Upton's data, which are excellent, the delivery of 300 rads protracted, at 0.86 rads per day means 300/0.86=349 days after initiation of experiment at 60 days of life to deliver the dose to the mouse. Since this is ~½ the life span of the mouse, the correspondence in humans would require spreading the dose out to 35 years or later. If we did this, we would be delivering most of the dose in the lower sensitivity adult period compared with the very sensitive childhood period. The calculation would be straightforward, and would lead to an enormous difference between acute and protracted radiation. Upton also found that as he moved up beyond 300 rads (>some 20 doubling doses) the leukemia incidence began to decline. We suspect this means the cells were *effectively* so aged they could no longer respond with leukemogenesis. Human evidence indicates that malignancy curves slow their rise at very advanced ages. Upton proved (see p. 27) decreased sensitivity to radiation leukemogenesis beyond 70 days in mice.

In summary, this analysis of low LET radiation indicates that *protracted* radiation does precisely what it is expected to do when account is taken of the *radiation being delivered later in life*, when the doubling dose is higher than in childhood. Actue radiation also does what is expected—when all of it is delivered in early childhood, the carcinogenesis is severe, just as is predicted.

It may be pointed out that in almost all the literature, experiments showing the illusory protection by fractionation, the *acute* irradiation

is performed early in life and the *protracted* irradiation is started at the same point, but continued into a much later part of the life span. One wonders why the experimenters, in the name of thoroughness, do not always do the additional acute exposure at the *end* of the protraction period *as well as* at the beginning. We suspect a psychologic factor may operate here. In any event, this issue of the greatest relevance in our considerations. Let us calculate the expected results. The data for 100 rads total exposure of low LET radiation can be utilized to test this crucial issue. In Table 3 a comparison is made:

100 rads delivered acutely in the 8th year of life,

100 rads delivered, protracted, at 10 rads per year from the 8th through the 17th year of life, inclusive.

We must now add the calculation for 100 rads delivered acutely *at the end* of the protraction period, namely at 17th year of life.

From Figure 1a, we have 1 rad=0.158 years of aging.

Therefore, 100 rads=15.8 years of aging.

Now we can calculate the expected cancer mortalities for all three groups at a chronological age of 40 years.

Radiation group	Effective age	Cancers per 10^6/year
100 rads delivered acutely (8th year of life)	69.0	9,750
100 rads delivered at 10 rads per year protracted (8th to 17th year of life)	61.7	5,750
100 rads delivered acutely (17th year of life)	55.8	3,540
Spontaneous	40.0	550

Note: The protracted radiation produces a much higher cancer mortality (5,200 excess cases per 10^6/year) than the same total dose delivered acutely in the 17th year (2,990 excess cases per 10^6/year). If most experimenters had delivered their acute radiation dose at the end of the protraction period rather than at the beginning, the literature would by now be filled with a different illusion—namely, that protracted radiation is more carcinogenic than acute radiation.

The view that is correct, in all probability, is that fractionation has nothing at all to do with the carcinogenic effect, *if* due account is taken of the sensitivity of the cells to radation varying with age of the experimental animal or human. Again, this simply points out the great hazard of irradiation early in life.

Thus, all the "protection" afforded by protraction in such experiments is illusory. There is not one shred of evidence requiring "repair" mechanisms to be operative to explain the observations. And as we shall see below, the consideration of high LET radiation makes these comments even more relevant.

High LET radiation

The Explanation of why even *Protracted High LET Radiation* is as Effective (or more) as *Acute Low LET Radiation.*

High LET Radiation Interaction with Matter

Many mysterious explanations have been suggested for the inordinately high effectiveness of high LET radiation, such as greater density of ionization along the tracks being more damaging, or less likely to be "repaired". We shall require none of these mysterious explanations and base our analysis only upon:

(*a*) Linear hypothesis, indicating *risk per rad* is the same for high LET radiation as for low LET radiation

plus

(*b*) The *physics* of the interaction of high LET radiation wit matter.

For illustrative purposes, we shal use *5 MEV Alpha Particles.* (On could study fast neutrons, protons or deuterons just as well).

Let us consider two sizes of cells, 10 microns in diameter, and 2 microns in diameter. This pretty well covers the general classes of mammalian cells. And as we shall see, the cell diameter *is* critica with respect to the effect of high LET radiation.

10 micron diameter cells interacting with 5 MEV α particles

As before, 1 rad $=6.25\times10^7$ MEV/gram

For 5 MEV α particles, this means $\frac{6.25\times10^7}{5}=1.27\times10^8$ α particles gram

For 10 micron diameter, cell volume $=4/3\pi(5)^3=523.7\mu^3$.

$\therefore$ Cells per gram tissue $=\frac{10^{12}}{523.7}=1.910\times10^9$ cells.

Range, *in tissue*, of 5 MEV α particles=45 microns ([12]).

Therefore, average alpha particle traverses $\frac{45}{10}=4.5$ cells.

For 1.25×10^7 α particles per gram corresponding to 1 rad, then $1.25\times10^7\times4.5-5.62\times10^7$ cells traversed.

But 1 gram of cells $=1.91\times10^9$ cells.

Therefore, 1 rad can provide α particles traversing only

$$\frac{5.625\times10^7}{1.910\times10^9}=0.02943, \text{ fraction of cells.}$$

Note: The delivery of 1 rad of 5 MEV alpha particles leaves 97.06% of the cells receiving *no* radiation. Thus, this represents highly *uneven* irradiation, and this is of the *utmost* importance in explaining the effects observed.

Furthermore, we must now give consideration to the Bragg curve for specific ionization along the 5 MEV alpha particle track. From Lapp and Andrews([12]), we can say, approximately, that for 5 MEV alpha particles, the ionization per unit path is $2x$ as high in the last half of the range as in the first half. Therefore, even for those cells that are traversed, one-half of them get twice the dosage of the other half.

For 1 rad, we saw above that 0.02943 is the fraction of 10 micron cells traversed.

Thus $(\frac{1}{2})(0.02943)=0.014715$ is the fraction receiving twice the dose received by the other 0.014715 fraction.

Let x=the dose in rads received by the cells in the first half of the range for total delievery of 1 rad of 5 MEV α particles.

Then $2x$=the dose in rads received by the cells in the second half of the range.

The vast bulk of cells (fraction=0.9706) receive no dose. Now calculate x, for the delivery of 1 rad overall.

$$(0.014715)(2x)+(0.014715)(x)+(0.97057)(0)=1.0$$

$$(0.044145)(x)=1.0$$

$$x=\frac{1}{0.044145}=22.65 \text{ rads}$$

$$2x=45.30 \text{ rads.}$$

Therefore, the delivery of 1 rad of 5 MEV α particles give rise to a population of cells which to a very close 1st approximation is as follows:

0.014715 of the cells receive 45.30 rads as dose.
0.014715 of the cells receive 22.65 rads as dose.
0.97057 of the cells receive No irradiation.

Now, for the successive delivery of 1 rad followed by another and another, we must apply *this same* distribution to the members of each of these 3 populations. We shall go through this for a total dose of 2 rads and of 3 rads to illustrate the principles. For more extensive dosage, either some equations will be utilized or a computer iteration performed. For now, the study of 1 rad, 2 rads, 3 rads will suffice, including a study of carcinogenesis by *acute* low LET radiation versus high LET radiation.

So, for 1 rad total we have the distribution above. Now let us add the 2nd rad to each population:

The 0.014715 fraction of cells having received 45.30 rads:
(0.014715) (0.014715) = 0.0002165 receive 45.30 rads *more*.
(0.014715) (0.014715) = 0.0002165 receive 22.65 rads *more*.
(0.014715) (0.97057) = 0.0142819 receive no additional radiation.

Now go on to the fraction of cells that had received 22.65 rads at the first rad overall

The 0.014715 fraction of cells having already received 22.65 rads
(0.014715) (0.014715) = 0.0002165 of cells receive 45.30 rads more.
(0.014715) (0.014715) = 0.0002165 of cells receive 22.65 rads more.
(0.014715) (0.97057) = 0.0142819 of cells receive no additional radiation.

Lastly, the
0.97057 fraction of cells having received 0 rads during 1st rad overall
(0.97057) (0.014715) = 0.0142819 of cells receive 45.30 rads more.
(0.97057) (0.014715) = 0.0142819 of cells receive 22.65 rads more.
(0.97057) (0.97057) = 0.9420060 of cells receive no additional radiation.

Now let us total these up, after 2 rads overall
0.0002165 + 0 = 0.0002165 of cells have received 90.60 rads.
0.0002165 + 0.0002165 = 0.0004330 of cells have received 67.95 rads.
0.0142819 + 0.0002165 + 0.0142819 = 0.0287803 of cells have received 45.30 rads.
0.0142819 + 0.0142819 = 0.0285638 of cells have received 22.65 rads.
0.942006 = 0 = 0.09420060 of cells have received no radiation.
Total = 1.000,000.

2 rads overall

Fraction of Cells:	Dose
0.0002165	90.60 rads
0.0004330	67.95 rads
0.0287803	45.30 rads
0.0285638	22.60 rads
0.9420060	0 rads.

Now we can consider each of these sub-populations when the 3rd rad is added.

The 90.60 rad group
(0.0002165) (0.014715) = 0.00000319 receive additional 45.30 rads.
(0.0002165) (0.014715) = 0.00000319 receive additional 22.65 rads.
(0.0002165) (0.97057) = 0.00021013 receive no additional radiation.

The 67.95 rad category
(0.0004330) (0.014715) = 0.000006372 receive additional 45.30 rads
(0.0004330) (0.014715) = 0.000006372 receive additional 22.65 rads
(0.0004330) (0.97057) = 0.000420257 receive no additional radiation

The 45.30 rad category
(0.0287803) (0.014715) = 0.0004235 receive additional 45.30 rads
(0.0287803) (0.014715) = 0.0004235 receive additional 22.65 rads
(0.0287803) (0.97057) = 0.0279333 receive no additional radiation

The 22.65 rad category
(0.0825638) (0.014715) = 0.0004203 receive additional 45.30 rads
(0.0285638) (0.014715) = 0.0004203 receive additional 22.65 rads
(0.0285638) (0.97057) = 0.0277232 receive no additional radiation

The 0 rad category
(0.9420060) (0.014715) = 0.0138616 receive additional 45.30 rads
(0.9420060) (0.014715) = 0.0138616 receive additional 22.65 rads
(0.9420060) (0.97057) = 0.9142828 receive no additional radiation

Now, after 3 rads, we have a population distribution of the following doses, which we must calculate by combining groups above:
135.90 rad category
113. 25 rad category
90.60 rad category
67.95 rad category
45.30 rad category
22.65 rad category
0 rad category

The 135.90 Category
0.00000319 fraction of cells

The 113.25 Category
0.00000319 + 0.00000637 = *0.00000956* fraction of cells

The 90.60 Category
0.00021013 + 0.00000637 + 0.00042350 = *0.00064000* fraction of cells

The 67.95 Category
0.00042030 + 0.0004235 + 0.0004203 = *0.0012641* **fraction of cells**

The 45.30 Category
0.0279333 + 0.0004203 + 0.0138616 = *0.0422152* **fraction of cells**

The 22.65 Category

$0.0277232+0.0138616=$ *0.0415848 fraction of cells*

The 0 Category

$0.9142828+0=$ *0.9142828* fraction of cells

So, we have

3 rads overall

Fraction of Cells:	Dose (rads)
0.00000319	135.90
0.00000956	113.25
0.00064000	90.60
0.00126410	67.95
0.04221520	45.30
0.04158480	22.65
0.91428280	0

Now we can go on to consider cancer production for high LET and low LET radiation.

Cancer Calculations: (α particles vs low LET acute)

1 rad total:

Let us consider a child in the 8th year of life
Let us deliver 1 rad of low LET acutely
Let us deliver 1 rad of 5 MEV alpha particles
Let us use Figure 1a, which sets 1 rad = 0.29 effective yrs. of aging

Now compare low LET with high LET 5 MEV α particles

For 1 rad low LET the effective aging = 0.29 years.
For 1 rad of 5 MEV alpha particles we have a population distribution

0.014715 of cells received 45.30 rads. $\therefore$ (45.30) (0.29) = 13.14 years of aging
0.014715 of cells received 22.65 rads. $\therefore$ (22.65) (0.92) = 6.57 years of aging
0.97057 of cells received 0 rads. $\therefore$ (0) (0.29) = 0 years of aging

CONSIDER CANCER MORTALITY AT AGE 40, CHRONOLOGICAL

	Effective age	Fraction of cells	Cancer rate/10^6/yr.	Number of cancers (fraction times rate)
5 MEV α particles—1 rad	53.14	0.014715	2,740	40.32
	46.57	.014715	1,220	17.95
	40.00	.97057	550	533.81
Total	40.29	1.0000		592.08
Low LET radiation—1 rad acute	40	1.0000	570	570
Spontaneous			550	550

Therefore, low LET radiation produces 20 excess cancers
high LET radiation produces 42.08 excess cancers

$$\text{RBE}=\frac{42.08}{20}=2.10 \text{ times as high for high LET radiation.}$$

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Now go on to 2 rads total dose

For 2 rads low LET, the effective aging = 0.58 years.
For 2 rads of 5 MEV α particles we have a population distribution

0.0002165 of cells received 90.60 rads. $\therefore$ (90.60) (0.29) = 26.2 years of aging
0.0004330 of cells received 67.95 rads. $\therefore$ (67.95) (0.29) = 19.7 years of aging
0.0287803 of cells received 45.30 rads. $\therefore$ (45.30) (0.29) = 13.1 years of aging
0.0285638 of cells received 22.65 rads. $\therefore$ (22.65) (0.29) = 6.5 years of aging
0.942006 of cells received 0 rads. $\therefore$ (0) (0.29) = 0 years of agin

So, at chronological age 40 years

	Effective age	Fraction of cells	Cancer rate/10^6/year	Number of cancers (Fraction × Rate)
5 MEV α particles 2 rads	66.27	0.0002165	8,300	1.8
	59.17	.0004330	5,180	2.2
	53.14	.0287803	2,740	78.8
	46.57	.0285638	1,220	34.8
	40.00	.942006	550	518.1
Total				635.8
Low LET, 2 rads-acute	40.58	1.0000	591	59
Spontaneous	40	1.0000	550	55

Therefore, for 2 rads:
low LET radiation produces 41 excess cancers
high LET radiation produces 85.85 excess cancers

$$\text{RBE}=\frac{85.85}{41}=2.09 \text{ times as high for high LET}$$

Now go on to 3 rads total dose

For 3 rads of low LET radiation, the effective aging = 0.87 years.
For 3 rads of high LET α particles we have a population distribution:

0.00000319 received 135.90 rads. $\therefore$ (135.90) (0.29) = 39.41 years of aging
0.00000956 received 113.25 rads. $\therefore$ (113.25) (0.29) = 32.84 years of aging
0.0006400 received 90.60 rads. $\therefore$ (90.60) (0.29) = 26.27 years of aging
0.00126410 received 67.95 rads. $\therefore$ (67.95) (0.29) = 19.71 years of aging
0.04221520 received 45.30 rads. $\therefore$ (45.30) (0.29) = 13.14 years of aging
0.04158480 received 22.65 rads. $\therefore$ (22.65) (0.29) = 6.57 years of aging
0.91428280 received 0 rads. $\therefore$ (0) (0.29) = 0 years of aging

So, at chronological age=40 years

	Effective age	Fraction of cells	Cancer rate /10⁶/year	Number of cancers (fraction × rate)
5 MEV α particles 3 rads	79.41	0.00000319	14,600	0.05
	72.84	.00000956	11,750	.11
	66.27	.0006400	8,270	5.29
	59.71	.00126410	5,180	6.55
	53.14	.04221520	2,730	115.25
	46.57	.04158480	1,210	50.32
	40.00	.91428280	550	502.86
Total				680.43
Low LET 3 rads, acute	40.87	1.0000	611	611
Spontaneous	40.00	1.0000	550	550

Therefore, for 3 rads:
low LET radiation produces 61 excess cancers
high LET radiation produces 127.28 excess cancers

$$RBE = \frac{130.43}{61} \quad 2.14 \text{ times as high for high LET radiation.}$$

Special points of note concerning high LET radiation-carcinogenesis

1. We have made no requirement that the high LET be acute or fractionated. If delivered in the same year of life, it wouldn't matter. This probably accounts for those experiments indicating *no* effect of *fractionation* for α particles.

2. The RBE in all 3 cases (1, 2, 3 rads) came out ~2.1 for α particles. However, these were all delivered in the 8th year of life. If we had spread the α particles radiation from 8th year through 18th year, we would be operating in a region where 1 rad is worth progressively less in terms of years of effective aging. The RBE of *this* type of "fractionation" (over 10 years) versus 8th year of age delivery of *acute* low LET radiation might come down to 1.0 or thereabouts. Starting at 2.10, we could afford several years of fractionation and still have the fractionated high LET radiation be *as effective as, or more effective* than *acute* low LET radiation for carcinogenesis.

3. Cell Size is extremely important in determining RBE. All the calculations above are for cells of 10 microns diameter. If we go to 20 microns diameter,

The number of cells traversed per α particle is ½ as many.
The number of cells per gram of tissue is ⅛ as many.
(Linear versus cube variation)

Therefore, the *fraction* of all cells traversed will rise, and hence the *uneven* distribution of radiation will be lessened. Since the greater efficacy of high LET radiation hinges on this *uneven* distribution of dose, the RBE would decrease for a tissue where the cells are larger compared with one where they are smaller. This is now demonstrated below.

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High LET Radiation in Cells with 20 microns diameter

For 20 microns diameter, cell volume $= \frac{4}{3}\pi(10^3) = 4179\mu^3$.

$$\text{Therefore, 1 gram of tissue has } \frac{10^{12}}{4.179\times10^3} = 2.39\times10^8 \text{ cells.}$$

$$\text{But 1 rad of MEV } \alpha \text{ particles represents } 1.25\times10^7 \frac{\alpha \text{ particles}}{\text{gram}}$$

$$\text{Each } \alpha \text{ particle traverses } \frac{45}{20} = 2.25 \text{ cells.}$$

Therefore, 1 rad of α particles provides traverse through $(1.25)(2.25)\times10^7 = 2.81\times10^7$ cells.

$$\text{Finally, fraction of cells traversed} = \frac{2.81\times10^7}{2.39\times10^8} = 0.1176$$

Fraction of cells not traversed = 0.8824.

What is the rad dosage to the cells traversed? Again, using the Bragg ionization for 5 MEV α particles, we can note that, of the cells traversed, ½ receive 2x the dose received by the other half. Proceeding as with cells of 10 microns diameter, let x = dose in rads received by the cells in the first half of the range

½ x 0.1176 = 0.0588 fraction of cells in first half of range
½ x 0.1176 = 0.0588 fraction of cells in second half of range.

Therefore,

$$0.0588(x) + 0.0588(2x) = 1.0$$
$$0.1764x = 1.0$$
$$x = 5.67 \text{ rads}$$
$$2x = 11.34 \text{ rads}$$

Thus, our distribution of cells after 1 rad delivery (for 20 micron cells) is as follows:

Fraction of cells	Dose (rads)	Effective aging (years)
0.0588	11.34	(11.34)(0.29) = 3.29
0.0588	5.67	(5.67)(0.29) = 1.64
0.8824	0	(0)(0.29) = 0

Now we can go on to calculate cancer mortality at chronological age 40 years for this distribution of cells and a total dose of 1 rad.

	(20-micron cell diameter)			
	Effective age	Fraction of cells	Cancer rate/ 10⁶/years	Number of cancers (fraction × rate)
5 MEV α particles—1 rad	43.29	0.0588	830	48.80
	41.64	.0588	650	38.22
	40.00	.8824	550	485.32
Total				572.34
Low LET Radiation—1 rad	40.29	1.000	570	570.00
Spontaneous	40.00	1.000	550	550.00

Therefore, 1 rad High LET Radiation produces 22.34 excess cancers
1 rad Low LET Radiation produces 20.0 excess cancers

$$RBE=\frac{22.34}{20.0}=1.12 \text{ times as high for high LET radiation.}$$

Contrasting this RBE=1.12 for cells of 20-micron diameter with the RBE=2.10 for cells of 10-micron diameter, the enormous influence of *cell size* becomes apparent. In the literature there is much puzzlement about the variation in RBE from experiment to experiment, often on different tissues or different animals. Unless *cell* sizes are known and accounted for, such variation in RBE is not surprising.

4. RBE will be *energy* dependent for the high LET radiation. For α particles of greater energy than 5 MEV, the fraction of cells receiving the full Bragg specific ionization effect will lessen. Hence, the uneven distribution of dose will be less than here calculated and the RBE will decline.

5. The RBE will, for any realistic cell size or energy of α particle, remain higher than unity, when the high LET radiation is distributed over time in the same manner as the acute low LET radiation.

6. None of these effects have anything to do with hypothetical "repair" mechanisms. They result only from unevenness of dose distribution for high LET radiation.

7. The RBE will depend upon the shape of the *spontaneous* curve of cancer mortality versus age. It can, therefore, be different for different cancers and even at different ages for the same cancer.

The answer to Dr. Storer's request for a factor of 5 reduction in carcinogenesis for fractionated low LET radiation versus acute low LET radiation

Storer has suggested that a five-fold "brownie point" allowance be made in our estimates of cancer mortality from radiation because low LET radiation fractionated is supposed to be less carcinogenic than acute low LET radiation (13). We hope the analysis of this report will demonstrate to Storer why we deny his fivefold factor. He has apparently been misled by the very point this analysis is all about—namely, that the fractionation "protection" is an illusion, reflecting only the fractionated radiation occurring *later* in life. The fractionated radiation distributed over a larger part of the life span produces as much carcinogenesis as is expected for the doubling doses at the various points in the life span. There is no reason whatever to bring in hypothetical "repair" since the observations are explainable without it.

The calculations presented here are for human protracted radiation between the 8th and 17th year of life, chosen to be comparable with the experiment of Upton on irradiation of RF male mice at 56 to 70 days of age. For our human analogy to accord with Upton's experiments it would be necessary to know that the effectiveness of radiation in the Upton experiment decreases, in terms of leukemogenesis per rad *beyond* 70 days of age of mouse. And indeed Upton did conclusively prove just that in a separate publication, with a demonstration that a marked decrease in leukemogenesis *per rad* occurs for radiation at 180 days of age versus irradiation with *the* same dose (300 rads x-irradiation) at 70 days (14). Thus, the analogy between our human calculation and Upton's mouse data is reasonable.

CONCLUSION

The unlikely prospect that protraction will in any way mitigate the carcinogenic effect of radiation in man

The experimental observation of "apparent" fractionation protection have here been explained away as an illusion, based upon delivering part of the fractionated radiation at a later time of life, when the radiation sensitivity is lower! No suggestion of "repair" is required or credible. What these studies *do* teach us is that the *younger* the individual, especially early childhood, the worse is the carcinogenic hazard per rad of radiation. It is bad to deliver radiation to individuals at 30 years of age, when the doubling dose is in the neighborhood of 50 rads; it is far worse to deliver it in infancy or childhood when the doubling dose is closer to 5 or 10 rads.

In our estimates of cancer production from population irradiation at FRC Guidelines we were conservative, using 100 rads as doubling dose and allowing *no* credit for the more serious effect per rad at earlier ages. Now, we realize the adult doubling dose may be closer to 50 rads, and further we should definitely credit the radiation as more carcinogenic for the early years of life. Both of these effects will materially increase our estimate above the 16,000 cancer cases previously predicted. And from what we now analyze concerning the illusion of protection by fractionation, one of the last hopes for mitigation of effect has all but evaporated.

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ICRP Publication 14

v.

The Gofman-Tamplin Report

(By Arthur R. Tamplin and John W. Gofman)

[This is Document No. 8 in a Series. Previously issued Nos. 3, 4, 6, and 7, 1–A.]

INTRODUCTION

Subsequent to the presentation of our testimony (The Gofman-Tamplin Report) before the Senate Sub-Committee on Air and Water Pollution, Committee on Public Works, we have obtained a copy of ICRP Publication 14. The purpose of this report is to discuss the similarities and differences between these two documents (1, 2).

It is shown that both documents come to the same conclusion and that the one major difference between the documents results from an oversight on the part of the authors of ICRP Publication 14.

LINEAR THEORY, THRESHOLD, LOW DOSE, LOW DOSE RATE

Gofman-Tamplin

We assume that the dose-response relationship is linear down to very low doses and dose rates. We contend there is no threshold. We show that these are more than reasonable assumptions in references 3 and 4.

ICRP Publication 14

The same assumptions have always been employed by the ICRP and are again used in Publication 14.

INDUCTION OF VARIOUS FORMS OF CANCER BY RADIATION, SIMILARITY OF THE DOUBLING DOSE

Gofman-Tamplin

We indicate that the data suggest that all forms of cancer can be induced by radiation. Furthermore, we indicate that the data suggest that, for a given dose of radiation, the various cancers will be induced in proportion to their spontaneous occurrence rate; i.e., if cancer X occurs 10 times more frequently than cancer Y, a given dose will induce 10 times more cancers of the X variety than of the Y variety.

ICRP Publication 14

The authors state that existing evidence does not confirm the validity of this concept. On page 23, it is stated: "It has been suggested that irradiation may have a multiplicative effect, a given exposure leading to the same proportional increase in incidence for a variety of different tumors independently of the actual levels of incidence in the absence of irradiation. Numerical estimates of the relative radiosensitivity to

tumour induction of all the different parts of the body would have been provided by such a general hypothesis but a survey of the available evidence did not confirm its validity."

In the purest scientific sense they are correct but they fail to state that the available data do not prove this concept to be wrong. Quite the contrary, the available data show this to be a significant public health concept. In fact, they present data which indicate that among the cancers studied, the great majority of them fit this hypothesis. Those which fit the hypothesis now represent some 90% of the expected cancer mortality. Table I is a reproduction of Table III.1 from page 60 of the ICRP Publication 14.

TABLE I

[Table III.1 from ICRP Publication 14]

[Cancer of heavily irradiated sites in ankylosing spondylitics at 1st January, 1963 (from tables III, VI, and VII of Court Brown, W. M., and Doll, R., 1965)]

	Number of cases observed	Number of cases expected	Excess over expected: Number of cases	Excess over expected: Rate per thousand persons
Subgroup in which the difference between the observed and expected cancer incidence was statistically significant ($P<0.025$ on a 1-tailed test):				
Leukemia	60	7	53.3	4.7.
Aplastic anemia	16	1	15.4	
Cancer of bronchus	96	54	41.8	2.9.
Other cancers (mostly carcinomatosis, primarily unknown)	24	7	17.2	1.2.
Cancer of stomach	38	24	14.4	1.0.
Malignant disease of lymphatic and hematopoietic tissues other than leukemia[1]	10	3	6.6	.3–.5.
Cancer of pancreas	12	6	6.3	
Cancer of pharynx	5	1	4.0	
Bone cancer	5	1	[2] 3.9	
Subgroup in which the difference between the observed and expected cancer incidence was not statistically significant:				
Cancer of:				
Ovary	4	2	1.8	(.8).
Larynx	2	2	0.2	.02 or less.
Esophagus	3	3	−0.4	
Skin	0	1	−1.4	
Hodgkin's disease	1	2	−1.5	
Cancer which may be clinically associated with ankylosing spondylitis: Cancer of colon	25	15	[3] 5.0	.4.

[1] Lymphosarcoma, reticulosarcoma, lymphoma unspecified (8 cases altogether) and myelomatosis (2 cases) as compared with 2.9 cases expected.

[2] The reliability of the diagnosis of primary bone tumors on a death certificate is not high. The excess of confirmed deaths due to bone sarcoma was 2.4.

[3] An excess of 10.2 was recorded but Court Brown and Doll (1965) reckoned that at least ½ of the excess might be attributed to the associations of cancer of the colon with ulcerative colitis and of ulcerative colitis with ankylosing spondylitis.

They make the following statements on page 61 of Publication 14 concerning the data in this table.

"The data on cancer incidence in different organs can be used to assess the relative sensitivity of the different organs to cancer induction if the ratio of the doses received by the different organs is known. Dolphin and Eve (1968) [1] deduced that the dose in the stomach was about 10 per cent of the overlying spinal marrow dose and that the mean dose to the stomach was about 7 per cent of the mean spinal marrow dose because the dorsal spine was sometimes not in the radiation field.

[1] Reference 6 of this report.

Let it be assumed that the leukaemia occurring in ankylosing spondylitics was due to direct irradiation of the bone marrow just as cancer of the stomach was due to direct irradiation of that stomach. The dose ratio for bone marrow/stomach was 100/7, whereas the ratio of induced malignancies for bone marrow/stomach was 4.7. The irradiated spinal morrow constituted about 40 per cent of the total active marrow (ICRP Publication 8, 1966) so that the relative sensitivities of bone marrow and stomach to cancer induction by irradiation were in the ratio

$\frac{4.7}{0.4}\times\frac{7}{100}=0.8$. Thus the data at present available can be interpreted

to suggest that the stomach and the whole bone marrow may have approximately the same sensitivity (Dolphin and Eve, 1968).[1] Under the same conditions of spinal irradiation the dose to colon, pancreas, bronchi or pharynx is likely to be similar to the dose to stomach and, judging by the observed excess of tumours, these organs may also seem to have a similar sensitivity to each other and to bone marrow and stomach."

The authors of ICRP Publication 14 seem to have overlooked the implications of the above paragraph. Since the dosage to the other organs was only 7% of the dosage to the spinal marrow, if the dosages were comparable, 14 times as many cancers would have been induced in the other organs. In other words, at the same dosage, the number of induced cancers at other sites would be proportional to their spontaneous occurrence rate. This is illustrated in Table II where we have corrected the excess leukemia cases to correspond to irradiation of the entire marrow by dividing the observed excess by the fraction of the marrow irradiated (53.3/0.4).

TABLE II.—DATA FROM TABLE III.1 OF ICRP PUBLICATION 14 CORRECTED FOR DIFFERENCE IN DOSAGE

Cancer	Spontaneous incidence (number cases expected)	Excess over expected number of cases	Ratio, excess/spontaneous
Leukemia	7	133	19.0
Bronchus	54	585	10.8
Other cancers	7	241	34.6
Stomach	24	202	8.4
Lymphatic and hematopoietic	3	93	31.0
Pancreas	6	88	14.7
Pharynx	1	56	56.0
Bone	1	10	10.0
Colon	15	140	9.3

The same type of correction was applied to the bone. The excess of the remaining cancers was multiplied by 14 to correct for the dosage difference. We have used all of the excess colon cancers. In Table II, except for cancer of the pharynx, all of the ratios (excess/spontaneous) fall within a factor of 2 of the value 17. Because of the closeness of the pharynx to the cervical spine, the factor of 14 was most likely too large a correction. The same might be true for the lymphatic hematopoietic tissues and possibly the group designated as the other cancers. In short, Table II indicates, as we suggested, that the dosage required to double the spontaneous incidence of these diverse forms of cancer is similar. We have presented evidence elsewhere (4,5) which shows that

the same is true for breast and lung cancer. This relationship is also established with thyroid cancer. In other words, this relationship is supported by the data for those cancers which comprise 90% of cancer mortality. Certainly this is a significant concept in terms of setting public health standards. The following section on latency discusses one reason why the remaining 10% of the cancer mortality may not yet be shown to fit this concept.

THE IMPORTANCE OF LATENCY

Gofman-Tamplin

We stated that the earlier interpretations of the ABCC data and other data were incorrect because many of the major forms of cancer were still in their latency period. We indicated that the most important data from the ABCC studies were just beginning to be collected. We indicated that as the observation period was extended, the other forms of cancer would begin to dominate the leukemia cases.

ICRP Publication 14

The authors of Publication 14 make the same conclusions. They state on page 22: "It had also to be recognized that the time which has elapsed since exposure is still much too short for it to be possible to assess the full tumour incidence in the spondylitics and the Japanese: the following table shows that evidence collected during the first 15 years or so after exposure could be regarded as covering only the beginning of the period in which neoplasms other than leukaemia might be expected to appear. If so, relatively small differences in the latent period of neoplasms arising in different tissues could lead to quite erroneous ideas about relative tissue susceptibility."

CHANGE IN RATE OF INDUCED MALIGNANT DISEASE WITH DURATION OF TIME SINCE EXPOSURE IN IRRADIATED ANKYLOSING SPONDYLITICS

[From data in table VI of Court, Brown, and Doll, 1965]

Years after irradiation	Cases per 10,000 man-years at risk: Leukaemia aplastic anemia	Cases per 10,000 man-years at risk: Cancers at heavily irradiated sites
0 to 2	2.5	3.0
3 to 5	6.0	.7
6 to 8	5.2	3.6
9 to 11	3.6	13.0
12 to 14	4.0	17.0
15 to 27	.4	20.0
Total of expected cases in 10,000 persons in 27 years calculated from the rates given	67.0	369.0

DOUBLING DOSE FOR CANCER INDUCTION

Gofman-Tamplin

We stated that the use of the doubling dose is a valid approach for estimating cancer induction by radiation. It was definitely a valid approach in our estimates of cancer induction in the United States population since the doubling dose was derived from data where the spontaneous incidence was comparable to that in the United States.

ICRP Publication 14

The following is taken from page 58 of Publication 14. "In radiological protection the radiation dose required to double the natural cancer incidence is sometimes used in assessing acceptable risks from somatic exposure by analogy with the concept of doubling dose used in assessing the genetic risks from exposure of the gonads. This concept of doubling dose for somatic hazards is a specific example of the misuse of the ratio of cancer rates. The natural incidence of stomach cancer in men or women in five different countries varies between 65 and 706 per million living (Segi and Kurihara, 1963, cited by Dolphin and Eve, 1968) so that for a fixed risk per rad the doubling dose varies more than ten-fold and will induce between 65 to 706 additional cases of stomach cancer per million persons depending on the particular population to which attention happens to be drawn."

Although it is not stated in the above quotation, Dolphin and Eve (6) point out that using a fixed risk per rad to calculate a doubling dose includes the assumption, "that the agents which produce the differences in the natural incidence of cancer of the stomach between countries do not act synergetically with radiation and thus cause changes in radiation sensitivity of the stomach." In other words, it is quite possible that the same dosage can produce 65 cancers per million in one country and 706 cancers per million in another country. The data on respiratory cancer in uranium miners shown in Table III illustrate this quite well (7).

TABLE III.—RESPIRATORY CANCER DEATH IN URANIUM MINERS

	Smokers	Nonsmokers
Person years	26,392	9,047.0
Cancers observed	60	2.0
Cancers expected	15.5	.5

Table III shows that radiation induced a four-fold increase in cancer in non-smokers and that acting synergetically with smoking, radiation induced the same four-fold increase in cancer in the smokers. The rate was increased by the same proportion in both populations although the spontaneous rates differed by a factor of 10.

SHOULD THE FRC GUIDELINES BE REDUCED

Gofman-Tamplin

We stated that, since the present data strongly suggested that all forms of cancer would be induced by radiation in proportion to their spontaneous occurrence, the FRC guideline of 170 mrem/yr exposure of the population-at-large should be reduced by at least a factor of 10.

ICRP Publication 14

In Appendix IV on page 114 using the marrow dose limit as an acceptable standard, they indicate that the whole body dose limit (it is now equal to the marrow dose limit) should be reduced to 0.14 to 0.17 of the marrow dose limit. They state, "On the other hand, the induced cancer rates in spondylitics for the first 27 years after first expo-

sure (Table 2, page 22) may suggest (with considerable statistical uncertainty) that the number of other fatal malignancies will be 5–6 times the number of leukemias. If so, the dose limit for uniform whole body exposure should be $\frac{1}{1+(5 \text{ or } 6)}$ i.e. 0.14—0.17 units."

In making this calculation they did not correct for the lower dosage received by the other organs in comparison to the bone marrow. The corrected data in Table II of this report show that the results of the proper calculation would have been $\frac{1}{1+(10 \text{ or } 11)}=0.083$ or 0.091

On page 115 they indicate that genetic consideration should further reduce the whole body dose limit. In other words, they are suggesting at least a factor of 10 reduction also.

SUMMARY

There is substantial agreement between Gofman-Tamplin and ICRP Publication 14. We believe, however, that they missed the important implication of the spondylitic data (see page 5) and as a result are underestimating the effects of radiation on major cancer sites other than the marrow by a factor of 2 to 3.

Since the ICRP is not a standard setting body but mainly a group that makes recommendations to such bodies, it is reasonable for them (and even essential) to qualify risk estimates in terms of absolute scientific validity. It may also be reasonable for them to await fairly substantial scientific evidence before making a recommendation.

However, the same does not apply to the standard setting bodies such as the Federal Radiation Council. Those bodies who are directly charged with protecting the public health should by necessity be conservative. In interpreting the available data, they should always weigh their analysis in favor of the public health.

As the follow-up time of the irradiated populations is being extended, the new data are demonstrating that the major forms of human cancer are being induced in proportion to their spontaneous occurrence rate. This strongly suggests that the dose limit to all organs should be comparable to that of the bone marrow or even lower and that the wholy body dose limit should be at least a factor of 10 lower than the marrow dose limit. The ICRP may be able to wait 10 to 20 years to make such a recommendation but the health of the public and the workers in the nuclear industry cannot wait.

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Major Fallacies in the AEC Staff Comments on the Gofman-Tamplin Papers and Congressional Testimony

I. The Demonstrated Validity of the Doubling Dose Concept as Used by Gofman and Tamplin

(By John W. Gofman and Arthur R. Tamplin)

[This is Document No. 9. Previously issued: Nos. 3, 4, 6, 7, 1-A, 8.]

INTRODUCTION

In a recent publication entitled "AEC Staff Comments on Papers and Congressional Testimony by Dr. John W. Gofman and Dr. Arthur R. Tamplin" (1), the following statement is made by AEC Staff.

"The concept of a doubling dose as applied to carcinogenesis by Gofman and Tamplin is here reviewed in detail and found to be without scientific validity".

This remarkable statement would be interesting if it were true. However, one could search the AEC Staff document for an infinite period and still not find the proof of this heralded finding that "the concept of a doubling dose . . . is found to be without scientific validity". Indeed, so unsure of itself is the AEC Staff that on the very next page of their document *they* state,

"Whether they (Gofman and Tamplin) are justified in evaluating hazard by use of the 'double dose' is a matter of opinion". (1)

This is indeed strange. First the AEC Staff finds the concept without scientific validity, and then backs off and indicates the concept's validity to be "a matter of opinion". Surely, if the AEC Staff *had* found the concept to be without scientific validity, that should *end* the matter. How is there still room for "opinion" in a concept that the AEC Staff has already proved invalid on *scientific* grounds? Could it be that the AEC Staff is ambivalent on this matter, and thus says, "Yes, No, and Maybe" all at once?

Whatever may be the basis for the AEC Staff position of vacillation, we propose to show here the following:

1. Scientific evidence *does* exist which clearly supports the validity of the "doubling dose" concept even *in two extreme tests.*
2. The AEC Staff position is not only erroneous, but in the field of radiation standard setting, this position can result in serious irresponsibility with respect to the public health.

THE SUBSTANTIVE PROBLEM RE DOUBLING DOSES

The only real challenge to the doubling dose concept is for a specific type of extreme case. Let us describe such a case, since the ICRP Task Force has alluded to it, and the AEC Staff has seized upon it. (2)

Suppose we consider stomach cancer incidence, in response to radiation. The ICRP Task Force quotes incidence rates (including males

and females) varying from 65 per 10^6 up to 706 per 10^6, among five different countries. The doubling dose concept would predict the following, if we used 1% increase in incidence rate per rad or 100 rads as the doubling dose:

For spontaneous incidence of 65 per 10^6/yr, the radiation-induced cases, for 100 rads, would be 65 per 10^6/yr.

For a spontaneous incidence of 706 per 10^6/yr, the radiation induced cases, for 100 rads, would be 706 per 10^6/yr.

On the other hand, if 100 rads induced a *fixed* number of cases (the ICRP Task Force assumption), say 65 per 10^6 per year, the radiation-induced cases would be 65 per 10^6 per year, in both situations. Obviously, there is a large difference between 65 additional cases and 706 additional cases. Which is correct?

In essence, this is a description of the central problem of cocarcinogenesis, namely *synergism* versus *additive* nature of co-carcinogenic influences. Now, what the carcinogenic influences are in one country versus another that lead to differing spontaneous incidence rates of a particular cancer, or in one group of people versus another in the *same* country, remain largely unknown. So we have no way of knowing whether any two potential carcinogenic influences should be additive or multiplicative. *Multiplicative* would be one expression, though not the only possible one, of synergistic action. It fits the doubling dose approach.

Suppose three influences were known, each of which could triple the cancer incidence rate in a population. If all three operate additively, the rate would go up $3+3+3=9$-fold. If all three operate synergistically in the multiplicative manner, the rate would go up $3\times3\times3=27$-fold, an enormously different answer.

As responsible public health officials, and in the *absence* of knowledge of additivity versus synergism, what should be assumed concerning such carcinogenic influences and exposure of hundreds of millions of people to them? Clearly, in the field of public health, our responsibility is to minimize hazard of death, and, therefore, where knowledge is yet incomplete, the *conservative* position is the only one that is a responsible position. Any other position is nothing short of playing Russian Roulette with the lives of humans. We choose not to play such Russian Roulette. The AEC Staff should think carefully about its views on this matter.

Indeed, the synergism assumption is *not even* necessarily the *most* conservative position. If a particular population shows a higher than usual cancer incidence rate spontaneously, they *may* be inordinately sensitive to additional carcinogenic effects. Thus radiation may be *even more* carcinogenic in such a population than the doubling dose concept predicts. In other words, the effect could be even more than mutiplicative. So, the doubling dose approach to this problem, while conservative, is by no means the most conservative. But it *is* far better than Russian Roulette with human lives.

Thus far, our considerations have been generalized, as though no solid scientific evidence could help solve the problem. But very solid scientific evidence *does* exist on this question of co-carcinogensis. It is indeed surprising to us that the AEC Staff launched an attack on synergism (which is what an attack on the doubling dose concept implies) without seriously considering the devastating human evidence

directly pertinent to our question. The first important study involves radiation as a human carcinogen plus cigarette smoking as a co-carcinogen. And this study deals with *the* most important form of cancer in men in the USA, namely bronchogenic lung cancer. The second important study involves asbestos exposure as a human carcinogen and cigarette smoking as a co-carcinogen—again for the all-important lung cancer.

Both studies, on *humans*, are highly significant and in close agreement pointing very *strongly* toward synergism between co-carcinogens, and away from additivity. Thus, in the radiation case, the evidence to be discussed below militates strongly in favor of the doubling dose concept and its implication of synergism. The asbestos exposure study points clearly in the same direction.

1. The Lundin-Archer Studies of Uranium Miners

In a continuing brilliant set of epidemiological investigations, Archer, Lundin, Holaday, Wagoner, and their collaborators have enormously advanced our understanding of lung cancer in man, and especially with respect to radiation-induction of lung cancer. Recently, Lundin and collaborators, and Archer and Holaday together, have published their experiences concerning mortality of uranium miners through September 1967.(3) Among the numerous extremely valuable contributions of this publication is a very important evaluation of the question of possible synergism between radiation and cigarette smoking among uranium miners.

For the period ending September 1967, these workers now have data on 62 cases of lung cancer having occurred among 3414 white uranium miners. They point out that it was technically feasible to define the uranium miners into two groups, smokers of cigarettes and non-smokers (this group including some who had smoked *some* cigarettes for less than 3 years). Utilizing the U.S. White Male rates for lung cancer together with standardized mortality ratios by smoking class, they were able to calculate the *expected* respiratory cancer deaths for the uranium miners, *if* radiation had had no effect. These expected values were then compared with the *observed* respiratory cancer deaths for smoking uranium miners and for non-smoking uranium miners. Since the radiation exposures are essentially the same for smokers and non-smokers, we have the direct comparison desired.

The Lundin-Archer data are reproduced here:

LUNDIN-ARCHER DATA

	Person-years	Respiratory cancer death	
		Observed	Expected
Cigarette smokers	26,392	60	15.5
Nonsmokers	9,047	2	0.5

Lundin and Archer concluded that radiation produces a 10-fold greater effect, in absolute terms, in cigarette smokers than in non-smokers, *which is precisely what synergism, on the doubling dose concept, would predict!*

Let us look at the data closely, for still further comment on the meaning of these very important findings:

In the non-smokers:

$2-0.5=1.5$ excess cases of cancer.

$\frac{1.5}{0.5}=3.0$ doubling doses, radiation-induced.

In the smokers:

$60-15.5=44.5$ excess cases of cancer.

$\frac{44.5}{15.5}=2.87$ doubling doses, radiation-induced.

Now 2.87 and 3.0 are so close together as to be regarded as identical. The 3.0 is, of course, based upon a small number of cases, so the statistics are not yet firm. But we shall show, even taking this into account, these data are *far* more in agreement with the doubling dose concept than with the *fixed* number of cases per unit radiation as suggested by the ICRP Task Force. Let us explore this in further detail.

In the Lundin-Archer data they show 26,392 person-years of exposure to uranium mining in the smokers and 9047 person-years exposure to uranium mining in the non-smokers. Therefore, according to the *opponents* of the doubling dose concept, if there are 1.5 excess cases (2–0.5) in the non-smokers, we would expect

$$\left(\frac{26892}{9047}\right)\times 1.5=(2.92)\,(1.5)=4.4 \text{ excess cancers in the smokers.}$$

but we have *44.5* excess cancers *observed* due to radiation in the cigarette-smoking. So the *opponents* of the doubling dose concept are off *by* a factor of 10. These data are enormously supportive of the doubling dose concept in two populations that differ spontaneously 10-fold in risk of lung cancer, namely cigarette-smoking uranium miners and non-smoking uranium miners. The failure to use the doubling dose concept has led here to an absurdity, predicting 4.4 excess cancers from radiation and observing 44.5 cases!

Let us go further and answer *in advance* those who point out that the total number of lung cancers in non-smokers is 2, and since this is a small number, the data may not be firm. This will only lead such skeptics into a worse quagmire.

The 0.5 value for *expected* cancers in non-smokers is sound, since it is based upon estimates from Haenszel for large numbers of cases.(4) It remains to consider the *observed* 2 cancers in the non-smokers. Because the number, 2, is small, it might truly be smaller or somewhat larger. It can't get much smaller—certainly it should truly be no lower than 0.5, which is the *expected* value. Let us suppose it is 0.5, for argument. This, however, would mean *no* radiation effect for non-smokers. Now let us consider our population of cigarette-smoking miners with approximately a 10-fold higher lung cancer incidence rate due to smoking. Here the observations are *very firm*, showing *44.5* excess cancers due to radiation. If we use *0* cancers, as above, as the radiation-excess for *non-smokers*, we would have to say radiation is infinitely worse in the cigarette smokers than in the non-smokers. So assuming 0 excess cancers in the non-smokers is not too reasonable. Suppose we assume the observed 2 cases should truly be 1 case. Then the excess in the

non-smokers, due to radiation, would be $1.0-0.5=0.5$ cases. Now, using the ICRP Task Force estimate, we should assign $(2.92)(0.5)=1.46$ *cases* as the radiation effect in smokers. But we *observe* 44.5 cases, or $\frac{44.5}{1.46}=30.5$ times as many! Clearly, the failure of ICRP to use the dose concept here would lead to an absurdity. The doubling dose concept would predict ~ 31 versus 44.5 observed.

Lastly, let us consider the small number, 2, were too low. Suppose it really might even be 4. Then radiation-induced excess cancers in non-smoking miners would be $4-0.5$, or 3.5 cases. Since the ICRP Task Force suggests a fixed number of cases for a given amount of radiation, we would expect $(2.92)(3.5)=10.2$ excess cancers in the cigarette-smoking uranium miners. But 10.2 is so far away from 44.5, as to consider the discrepancy results from an absurd assumption.

Thus, no matter how the small number, 2, would change in a larger series, we see that the ICRP Task Force's suggestion is very unreasonable for the uranium miner data of Lundin and Archer. The doubling dose concept predicts results in perfect harmony with the observations—and does so in precisely the kind of situation that the ICRP Task Force and AEC Staff are skeptical! The doubling dose concept *may* not be perfect, but it must be much closer to the truth than the ICRP Task Force approach.

Lundin and Archer point out that co-carcinogenesis between asbestos exposure and cigarette smoking provide data analogous to their own on co-carcinogenesis between radiation and cigarette smoking. Let us now examine the asbestos studies.

2. The Selikoff-Hammond-Churg Data on Asbestos Carcinogenesis

Selikoff and co-workers recently published extensive data on co-carcinogenesis between asbestos exposure and cigarette smoking in the induction of primary lung cancer and other cancers.(5) We shall restrict our considerations to bronchogenic lung cancer, for which their data are most extensive. There is no doubt that their on-going studies will later provide highly important data for other forms of human cancer, as well as for bronchogenic lung cancer.

Their studies are based upon essentially complete followup of 370 asbestos-insulation workers observed between January 1, 1963 and April 30, 1967. Cigarette smoking histories were obtained by these investigators for the entire group. In the followup period, 24 deaths due to bronchogenic carcinoma occurred in the 370 asbestos workers. The distribution of cases of lung cancer by smoking category is presented below. (From Table 5 of Reference 5.)

TABLE 5 (REFERENCE 5).—OBSERVED AND EXPECTED BRONCHOGENIC CARCINOMA DEATHS BY SMOKING HABITS FOR 370 ASBESTOS WORKERS

Smoking habits	Number of subjects	Observed deaths	Expected deaths
Never smoked regularly ... History of pipe, cigarsmoking only ...	87	0 / 0	0.05 / .13 (0.18)
History of regular cigarette smoking ...	283	24	2.98
Total ...		24	3.16

Let us now examine these data using the ICRP Task Force doctrine of "additivity", rather than the doubling dose concept. In the non-smokers of cigarettes group no cases were observed. According to ICRP Task Force, the same average exposure to asbestos should lead to 0 excess cases in the cigarette smokers who are exposed to asbestos. But the observed data show $24-2.98\cong21$ excess cases. Now the difference between 0 and 21 is mammoth, indicating the absurdity of the "additivity" principle (*no synergism*) of ICRP Task Force.

But let us say that we are dealing with small numbers, and hence the observed 0 cases in the non-smokers could truly be 1, or 2, or even 3. We have to start straining our credibility to go beyond 3 and still observe 0 cases. So we will try 3 cases. Now, there are 87 persons who never smoked cigarettes regularly, with an expected lung cancer incidence$=0.18$ cases. Having stretched our credibility, we assumed 3 observed instead of 0 which was observed. So the excess, due to asbestos exposure would be $3-0.18=2.82$ cases in 87 men. If the ICRP Task Force view were correct, asbestos exposure in 283 cigarette smokers should produce $\left(\frac{283}{87}\right)(2.82)=(3.46)(2.82)=9.75$ excess cancers. But the excess cancers observed$=24-2.98\cong21$ cases. Clearly the ICRP Task Force approach is far wide of the mark, and this is even after we stretched the 0 cases in the non-smokers to 3, to account for the possible effect of small-number statistics.

In contrast, the observations of the asbestos workers are completely in harmony with the doubling dose concept. If 21 excess cancers are observed in the asbestos-exposed cigarette smokers, $\frac{21}{2.98}=7.05$ doubling doses. Therefore, in the combined non-smokers of cigarettes (including cigar+pipe smokers), we expect $(7.05)(0.18)=1.26$ cases, due to asbestos exposure. The total expected would be $1.26+0.18=1.44$ cases. The observed was 0 cases, and these are consistent, for the small numbers. Thus, with 1.44 expected, 0, 1, 2, or 3 occurring would not be at all unusual on a random basis. If anything, the observation of less than the 1.44 expected would indicate the doubling dose concept is conservative in its predictions for the effect of asbestos exposure on cigarette smoking, not radical.

In view of these two major and well-executed studies showing the doubling dose concept in harmony with observation when two populations differing 10-fold in "spontaneous" risk are exposed to a co-carcinogen (*radiation in one case, asbestos exposure in the other*), and in view of the marked disagreement the ICRP Task Force position leads to, perhaps that Task Force and AEC Staff may wish to reconsider their position, now so very shaky.

3. Are There Any Cases of Co-Carcinogenesis in Humans Where the Doubling Dose Concept (Synergism) Doesn't Apply?

We have just presented data from two beautifully executed studies of co-carcinogenesis, one for radiation and cigarette smoking, the other for asbestos exposure and cigarette smoking. Both are clearly and unequivocally in harmony, at a minimum, with the doubling dose concept and synergism. Both are *violently* and *grossly inconsistent* with the additivity principle espoused by the ICRP Task Force. We are at a complete loss to understand how the ICRP Task Force in 1969 flatly "assumes no synergism." (2)

We would gladly admit that, in principle, situations may indeed exist where synergism between co-carcinogens *may* not operate. To *assume* in a particular case it doesn't operate is sheer public health irresponsibility, especially where two major studies on humans clearly show it does!

Possibly the ICRP Task Force and the AEC Staff may be thinking about the observations of Jablon concerning stomach cancer in Hiroshima-Nagasaki atom bomb survivors.(6) We shall now consider that study, for it is assuredly a genuine red herring in the entire issue of human radiation carcinogenesis. Jablon examined "cancer of the digestive organs and peritoneum" as a cause of mortality during the 1950–1960 period in Japanese survivors of the atomic bombing. As any student of human radiation carcinogenesis knows, the real peak in incidence of radiation-induced cancers *other than leukemia* is between 15 and 30 years after exposure, and this peak differs for different cancers and for different dosages. Now, since the atomic bombing occurred in 1945, the data collected by Jablon for stomach cancer are for the period 5 to 15 years post-radiation. Since extremely few of the radiation-induced cancers can be expected at all in the first 10 years post-exposure, we can estimate that the largest part of the period of observation in the Jablon studies is not only irrelevant, it is *positively* deceiving. For the *larger* the part of the observation period during which the radiation-induced cancers cannot occur, the *more* the radiation cases are *diluted* by spontaneous cases, and the *more* the entire effect is obscured. Assuredly, Jablon and co-workers had not the least intention of obscuring anything. They simply were reporting the data then available. But the *real* data one needs to see for Hiroshima-Nagasaki must be those where the *largest part* of the observation period is *beyond* 1955, and those data are not yet available in published form so far as we know.

In a personal communication to R. Batzel, B. Shore, E. Fleming and one of us (J. W. Gofman), Dr. John Totter reported to this group in 1969 that the stomach cancer data in Japan now clearly show a significant association with radiation. To ascertain whether synergism and the doubling dose concept fit this group (with a higher-than-usual spontaneous stomach cancer incidence), we simply must have the recent data from Japan. So far our efforts to obtain these particular ABCC data have been fruitless. We do hope they will be published soon. In the meantime, any comfort to be obtained either by the ICRP Task Force or AEC Staff should, above all, not rest upon the irrelevant studies of stomach cancer in Japan for the period 1950–1960, since it is so obviously diluted by a large period where radiation-induction wouldn't have been at all appreciable.

CONCLUSIONS

1. The Doubling Dose Concept (a manifestation of synergism) for Radiation-Carcinogenesis in humans has proved sound in a major study by Lundin and Archer where a sub-population of low risk (non-smokers) and a sub-population of high risk (10 times as high) (cigarette smokers) are compared with respect to lung cancer induction by radiation. Furthermore, in a totally separate human co-carcinogenesis study, the doubling dose type of concept of synergism has proved sound where a sub-population of low risk (non-smokers) and

a sub-population of high risk (cigarette smokers) are compared with respect to lung cancer induction by asbestos exposure.

The ICRP Task Force *additivity* approach, namely, a fixed number of cases per rad of radiation, leads to absurd results in the radiation study, and its equivalent approach leads to absurd results in the asbestos exposure study. We suggest the Task Force beat a retreat, *or* produce *some* evidence supporting *their* dismissal of synergism.

The AEC Staff, following the ICRP Task Force, has produced some empty words in saying "the concept of a doubling dose as applied to carcinogenesis by Gofman and Tamplin is reviewed here in detail and found to be without scientific validity".

To use a favorite quotation of ours that the AEC Staff seems to enjoy, "We have produced here *hard*, incontrovertible data—*facts*, not opinions".

We invite some *facts*, some *hard*, incontrovertible data either from AEC Staff *or* the ICRP Task Force they quote so freely.

Even if someday a *specific* case of additivity is demonstrated, the already-existing major cases of synergism provide an enormous bulwark for the public health soundness of the synergism, or doubling dose, concept in *any new unknown situation.*

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Radiation Induction of Breast Cancer in the Rat

(A Validation of the Linear Hypothesis of Radiation *Carcinogenesis* over the Range 0–600 rads)

(By John W. Gofman and Arthur R. Tamplin)

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INTRODUCTION

The AEC Staff Document criticized our work on radiation carcinogenesis as having ignored a large body of experimental animal data—data they infer might have altered our estimate of 16,000 additional cancer deaths from U.S. population exposure at FRC Guidelines. (1) (2)

We, of course, always have considered, and always shall consider relevant experimental animal studies, for they do indeed provide valuable clues that may be important for radiation exposure of man.

However, our estimates were made utilizing human data, since humans are the relevant subjects of our concern. The three issues of importance, where animal data might help, are:

(*a*) The issue of a "safe threshold" of radiation.
(*b*) Linear versus non-linear dose response relationship.
(*c*) The issue of acute versus protracted radiation.

Actually (a) and (b) are parts of the same problem, namely, dose response relationships. In a separate report of this series we have dealt with the acute versus protracted dose delivery, and demonstrated that supposed protection through protraction of radiation, based upon excellent experimental animal data, is ***illusory***. Those experiments are much better interpreted as simply that radiation, in protracted experiments, is delivered later in life, when the value of each rad for carcinogenesis is less. (3) Thus, any hope for "repair" of carcinogenic damage from such studies, we believe, is quite ephemeral.

The issue of dose-response relationships is one where we have already examined considerable human data. Those data certainly present no scientific evidence suggesting any safe radiation threshold. (4) (5) (6) Indeed, both the Uranium Miner data and the breast cancer data in humans suggest, if anything, that the effect per rad of radiation is even worse at low doses than at high doses. Hempelmann, in a recent evaluation of radiation-induced tyroid adenomas, indicates linearity in the dose-response curve down to 20 rads total dose, with no suggestion whatever of any *safe* threshold. (7) And very recently Stewart has published evidence that there *is* a dose-response relationship for radiation-carcinogenesis in-utero based upon the number of films taken during obstetric radiography. (8) This would be in the neighborhood of 0 to 10 rads.

Overall, there is no evidence on humans that even remotely argu for any safe threshold with respect to radiation carcinogenesis. AE Staff suggests that one should study experimental animal data co cerning this issue. We agree, and indeed we have been preparing a extensive report on this very subject, including induction of cancer i various tissues. However, because of the timeliness, we shall prese the first section of that report here, based upon the elegant studies the first section of that report here, based upon the elegant studies mammary cancer induction by radiation in rats by Bond and his co laborators at the Brookhaven National Laboratory. This study repr sents an exhaustive important series of researches. The maj conclusion reached by Bond and co-workers is that the data show pe fect linearity in mammary cancer induction by radiation with x-ra all the way down to 25 rads, and that there is indication of linearity, l this small extrapolation, all the way to 0 rads. (9)

It is interesting that one of the most thorough studies of exper mental animal carcinogenesis leads to conclusions diametrically o posed to the AEC Staff position of safe thresholds of radiation.

While we would never be so arrogant as to extrapolate dose-respon relations from rodent to man for many reasons, the absence of scali parameters being a major one, it is of great interest to know t "ball-park" range for doubling doses for mammary cancer inducti in the rat as compared to those we have presented for radiatio induction of mammary cancer in humans. (6)

The data of Bond et al show that for 400 rads of radiation, t Sprague-Dawley rats show 80% of the animals to have at least o breast cancer by the end of the observation period of 11 mont Thus, 400 rads is already "a saturation" type of dose, and hen unsuitable for dose-response relationships in an 11-month observati period. However, it is possible to study the extremely high dose regi (400 rads or more) if the observations of breast cancer are made at much earlier period of life than 11 months. Fortunately, the ve thorough studies of Bond and co-workers provide data which allo calculation of mammary cancer incidence out to 6 months post-irra ation, and these observations will be used in the high-dose calculatio

In all estimates of doubling dose for radiation carcinogenesis, prime input is the *spontaneous* incidence of the particular can under study. If that cancer is spontaneously fairly rare, a large seri is required to provide a stable value for the spontaneous inciden Bond studied 77 rats without irradiation and observed *one* bre cancer. The statistics of small numbers is such that the true numb might be 2, or at the outside, even 3. We shall, therefore, make t calculations using the observed spontaneous incidence of 1 cancer 77 rats, and also provide an "extreme" analysis using the value 3 per 77 rats, as an outside limit.

The purpose of the analysis is to compare doubling doses f mammary cancer induction in rats at various parts of the entire sc of doses from 0 through 600 rads. This provides an excellent t of the doubling dose concept, and of linear theory, and furtherm can indicate whether any trends suggest any type of safe thresho As we shall see below, any trend is ***opposite*** to a threshold. Last we shall compare the doubling doses for mammary cancer inducti in rats with those we have already reported for humans in Hiroshi Nagasaki and Nova Scotia, Canada. (6)

The 0-200 rad region

We have taken all the prime data of Bond and co-workers relevant for our analysis and reproduced them in Table 1. We shall limit our analysis to the overall induction of mammary cancer. In our more detailed presentation of all experimental animal data, we shall later consider specific histologic types of cancer, such as adenocarcinoma and adenofibroma, as well as provide an analysis based upon total cancers induced, rather than number of rats developing cancer. As Bond et al have shown, multiple breast cancers are frequent in the irradiated animals.

TABLE 1.—THE PRIME INPUT DATA FOR THE STUDY OF RADIATION INDUCTION OF BREAST CANCER IN SPRAGUE-DAWLEY RATS[1]

Category	Radiation dose (rads)	Number of rats studied	Number of rats developing breast cancer— By 11 months	By 6 months
A	0	77	1	0
B	25	47	5	1
C	50	16	2	1
D	100	14	3	1
E	200	44	17	5
F	400	58	45	28
G	470	43	25	14
H	530	42	28	10
I	600	58	33	24

[1] From Bond et al., reference 8.

Spontaneous Breast Cancer rate in Sprague-Dawley rats (11 months of observation):

Observed, 1 cancer in 77 rats, or an incidence $=\left(\frac{1000}{77}\right)(1)$, or 13 breast cancers per 1000 rats.

Extreme estimate is 3 cancers in 77 rats, or an incidence $=\left(\frac{1000}{77}\right)(3)=39$ breast cancers per 1000 rats.

Categories B through E—including 25 rads, 50 rads, 100 rads, 200 rads: Mean Dose calculation is the first step. (See data, Table 1)

$$\text{Mean Dose}=\frac{(25)(47)+(50)(16)+(100)(14)+(200)(44)}{47+16+14+44}$$

$$=\frac{1175+800+1400+8800}{121}=\frac{12175}{121}$$

$\therefore$ Mean Dose $=100.6$ rads, for the overall group.
Rats developing mammary cancer $=5+2+3+17=27$ out of 121 animals.

This corresponds to an incidence of $\left(\frac{1000}{121}\right)(27)$, or 223.1 rats with breast cancer per 1000 animals observed.

DOUBLING DOSE CALCULATION

	Observed data	Extreme (outside) value
Irradiated rats	223.1 per 1,000	223.1 per 1,000
Spontaneous incidence	13.0 per 1,000	39.0 per 1,000
Excess, radiation-induced	210.1 per 1,000	184.1 per 1,000
Doubling doses =	$\frac{\text{Excess}}{\text{Spontaneous}}$	$\frac{\text{Excess}}{\text{Spontaneous}}$
	$\frac{210.1}{13.0}$	$\frac{184.1}{39.0}$
	16.2 doubling doses	4.72 doubling doses
So, 100.6 rads =	16.2 doubling doses	4.72 doubling doses
∴ One doubling dose =	6.2 rads	21.3 rads

So, for all categories out to 200 rads, the most probable doubling dose for mammary cancer induction by radiation is 6.2 rads, with a small likelihood it could be as much as 21.3 rads.

Categories B through D—including 25, 50, 100 rads, but excluding 200 rads:

$$\text{Mean Dose}=\frac{(25)(47)+(50)(16)+(100)(14)}{47+16+14}$$

$$=\frac{1175+800+1400}{77}=\frac{3375}{77}$$

$\therefore$ Mean Dose $=43.8$ rads, for the overall group.
Rats developing mammary cancer $=5+2+3=10$ out of 77 animals.

This corresponds to an incidence of $\left(\frac{1000}{77}\right)(10)=130$ rats with breast cancer per 1000 animals observed.

DOUBLING DOSE CALCULATIONS

	Observed data	Extreme (outside) value
Irradiated rats	130 per 1,000	130 per 1,000
Spontaneous incidence	13 per 1,000	39 per 1,000
Excess, radiation-induced	117 per 1,000	91 per 1,000
Doubling doses =	$\frac{117}{13}$	$\frac{91}{39}$
=	9 doubling doses	2.33 doubling doses
So, 43.8 rads =	9 doubling doses	2.33 doubling doses
∴ One doubling dose =	4.9 rads	18.7 rads

So, for all categories out to 100 rads, the most probable doubling dose for mammary cancer induction by radiation is 4.9 rads, with a small likelihood it would be as much as 18.7 rads.

Categories B+C, including 25 and 50 rads only:

$$\text{Mean Dose}=\frac{(25)(47)+(50)(16)}{47+16}=\frac{1175+800}{63}=\frac{1975}{63}$$

$\therefore$ Mean Dose $=31.3$ rads.
Rats developing mammary cancer $=5+2=7$ out of 63 animals.

This corresponds to an incidence of $\left(\frac{1000}{63}\right)(7)=111.1$ rats with breast cancer per 1000 animals observed.

DOUBLING DOSE CALCULATION

	Observed data	Extreme (outside) value
Irradiated rats	111.1 per 1,000	111.1 per 1,000
Spontaneous incidence	13.0 per 1,000	39.0 per 1,000
Excess, radiation induced	98.1 per 1,000	72.1 per 1,000
Doubling doses equals	$\frac{98.1}{13}=7.5$	$\frac{72.1}{39}=1.8$
So, 31.3 rads equals	7.5 doubling doses	1.8 doubling doses
Therefore 1 doubling dose equals	4.2 rads	17.4 rads

So, for the categories out to 50 rads, the most probable doubling dose for mamary cancer induction by radiation is 4.2 rads, with a small likelihood it would be as much as 17.4 rads.

Category B alone—25 rads Mean Dose—25 rads.

5 cancers developed in 47 animals. This corresponds to an incidence of

$$\left(\frac{1000}{47}\right)(5)=106.4 \text{ rats with breast cancer per 1000 animals observed.}$$

DOUBLING DOSE CALCULATION

	Observed data	Extreme (outside) value
Irradiated rats	106.4 per 1,000	106.4 per 1,000.
Spontaneous incidence	13.0 per 1,000	39.0 per 1,000.
Excess, radiation induced	93.4 per 1,000	67.4 per 1,000.

$$\text{Doubling Doses}=\frac{93.4}{13}=7.18 \quad \frac{67.4}{39}=1.7$$

So, 25 rads=7.18 doubling doses 1.7 doubling doses

$$\therefore 1 \text{ doubling dose}=\frac{25}{7.18}=\mathit{3.5\ rads} \quad \frac{25}{1.7}=\mathit{14.7\ rads}$$

We can now summarize all these doubling doses:

Categories	Mean dose (rads)	Most probable doubling dose (rads)	Extreme (outside) doubling dose (rads)
B+C+D+E (25,50,100,200 rads)	100.6	6.2	21.3
B+C+D (25,50,100 rads)	43.8	4.9	18.7
B+C (25,50 rads)	31.3	4.2	17.4
B (25 rads)	25.0	3.5	14.7

The data as a whole represent a beautiful confirmation of linear theory, and the doubling dose concept. Indeed, if anything, the radiation effect in producing breast cancer in rats is even *more* severe at low total doses than predicted by linear theory. This may be a real effect, or possibly only apparent due to mild saturation effects at the high doses.

The results are precisely the *opposite* of anything even remotely resembling a threshold. For a threshold to exist, the doubling doses, at

low total doses, should be going toward infinity. Instead the doubling dose is decreasing to below 4 rads! Even allowing for a higher spontaneous breast cancer incidence than Bond observed, the doubling dose appears to be below 20 rads, and again behaves precisely opposite to threshold concepts. Nor should this extreme sensitivity to breast cancer induction by radiation in rats be at all surprising. Thus, from Upton's data on mice, one calculates readily, for the 0–100 rad region the following: (10)

RF Male Mice (x-rays) Myeloid Leukemia: Doubling Dose—23.1 rads.

RF Female Mice (Co^{60} γ rays) Ovarian cancer: Doubling Dose=17.6 rads.

Many more data will be presented in our detailed further animal studies.

INDUCTION OF BREAST CANCER BY X-RAYS

The 400–600 rad region

We shall now calculate the doubling dose for the very high dose region, utilizing the mammary cancer incidence (of Table 1) for up to 6 months, to avoid the saturation phenomena encountered in the 11-month observations.

Again, we need the spontaneous incidence rate as a prime input. Bond observed 0 cancers in 77 rats out to 6 months. But because of small numbers, this 0 value would not hold up in a larger series. For all radiation categories as a whole, including unirradiated animals, there were 84 rats showing breast cancer by six months of age, whereas there were 159 rats (including the 84) showing breast cancer by 11 months. Therefore, an excellent approximation is that the breast cancer incidence is $^{84}/_{159}$, or 53% as high at 6 months as at 11 months. Because cancers appear earlier in irradiated animals, in general, the use of this factor will overestimate the spontaneous incidence at six months, and, hence, increase *apparent* doubling doses. Thus, the radiation effect will be *underestimated*, if anything.

For 11 months, we used 13 per 1000 as spontaneous incidence, and 39 per 1000 as an outside extreme.

(0.53)(13)=7.4 estimated spontaneous incidence at *6 months.*
(0.53)(39)=20.7 estimated extreme spontaneous incidence at *6 months.*

Categories F+G+H+I (400 rads, 470 rads, 530 rads, 600 rads) (See data of Table 1):

$$\text{Mean Dose}=\frac{(400)(58)+(470)(43)+(530)(42)+(600)(58)}{58+43+42+58}$$

$$=\frac{23200+20210+22260+34800}{201}=\frac{100470}{201}$$

$$\text{Mean Dose}=\mathit{499.9\ rads}$$

Rats developing mammary cancer in *6 months*=28+14+10+24=76 rats out of 201 animals. This corresponds to $\frac{1000}{201}$ (76)=378.1 per 1000 as the number of rats developing breast cancer in 6 months per 1000 animals observed.

DOUBLING DOSE CALCULATION

	Best estimate	Extreme estimate
Irradiated rats	378.1 per 1,000	378.1 per 1,000
Spontaneous incidence	7.4 per 1,000	20.7 per 1,000
Excess, radiation-induced	370.7 per 1,000	357.4 per 1,000
Doubling doses =	Excess/Spontaneous	Excess/Spontaneous
=	$\frac{370.7}{7.4}=50.1$	$\frac{357.4}{20.7}=17.3$
So, 499.9 rads =	50.1 doubling doses	17.3 doubling doses
∴ One doubling dose =	$\frac{499.9}{50.1}=10.0$ rads	$\frac{499.9}{17.3}=28.9$ rads

These values, 10.0 rads as best estimate and 28.9 rads as an outside value, are extremely consistent with all the data presented above for 200 rads and less. When we consider that (a) we have probably over-estimated the spontaneous incidence at 6 months, and (b) there may still be *some* saturation effects, even at 6 months, the doubling doses may very well be approximately constant over the entire range from 0 through 600 rads.

CONCLUSIONS

1. Bond and co-workers' excellent studies of mammary cancer induction by x-rays in Sprague-Dawley rats show, as Bond and co-workers indicated, a linear dose-response relationship with no suggestion of any safe radiation threshold. Our analysis of their data is in total accord with this view. If there is *any* trend, it is *away* from a threshold and suggests a somewhat higher risk of cancer per rad as lower and lower doses are approached.

2. The best estimate of the doubling dose for mammary cancer induction by x-rays in Sprague-Dawley rats is in the neighborhood of 5 rads. It is unlikely to be higher than 20 rads.

3. These doubling doses are remarkably close to those we have reported for women in Nova Scotia and in Japan, in both of which doubling doses were in the 25 rad region, over a large range of total doses.

4. These experimental animal cancer-induction studies are in excellent accord with the linear, non-threshold model of radiation carcinogenesis which fits the human observations so well.

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Radiation Aging by High LET Radiation: The Implications of Assuming Cell Nucleus Irradiation Is the Relevant Parameter

(By Donald P. Geesaman, Bio-Medical Research Division, Lawrence Radiation Laboratory (Livermore), University of California)

(A companion paper to "The Mechanism of Radiation Carcinogenesis" (Document 1–A) by John W. Gofman and Arthur R. Tamplin)

[This is Document No. 10. Previously issued: Nos. 3, 4, 6, 7, 1–A, 8, 9, 2–A.]

Introduction

In the companion paper to this one, Gofman and Tamplin have provided an explanation of R.B.E. for high LET radiation carcinogenesis based upon the uneven distribution of high LET radiation among the cells of a tissue at moderate radiation doses (1).

There is considerable evidence to suggest that the crucial events in carcinogenesis may occur in the cell nucleus, rather than in the cytoplasm. It is of interest to explore the consequences for R.B.E. if this is true.

We shall *assume* that energy delivered in the nucleus affects only the nucleus; energy delivered in the cytoplasm does *not* affect the nucleus. To what extent relevant free radicals, or other radiation-produced entities will migrate between cytoplasmic and nuclear volumes is unknown. We shall calculate the extreme case where no cytoplasmic-nuclear exchange of relevant radiation energy occurs. This, as we shall see, may considerably raise the potential R.B.E. of high LET radiation for carcinogenesis in comparison ith low LET radiation.

Description of the Physical Parameters

Assume:

(1) an idealized cubical cell
(2) Cell diameter = 2× nuclear diameter
(3) Cell volume = 8× nuclear volume

Next, assume a cubical array of cubical cells, and α particles incident along a normal axis of the cell.

Thus the fraction of cells in which the α particle will traverse the nucleus is directly the ratio, $\frac{\text{(nuclear cross-section)}}{\text{(cellular cross-section)}}$

But, since cell cross section = 4× nuclear cross section, it follows that ¼ *as many nuclei* are traversed by α particles in this idealized array *as are* cells.

The essential difference here, therefore, in contrast to the cellular treatment of Gofman-Tamplin is that ¼ as may nuclei are traversed, for any given number of α particles, as they estimated for cells.

Number of cells per gram = 1.91×10^9 For $523.7\mu^3$ volume)

For 5 MEV α particles

8.05μ cell diameter. ($523.7\mu^3$ volume)

5 MEV α particle Range = 45 microns in tissue.

Cells traversed per α particle (5 MEV) $= \frac{45}{8.05} = 5.6$ cells.

1.25×10^7 α particles correspond to 1 rad

Then $(1.25\times10^7)\ (5.6) = 7.0\times10^7$ cells traversed

Therefore 1 rad provides α particles traversing,

$\frac{7.0\times10^7}{1.910\times10^9} = 0.0366$, fraction of the cells.

If ¼ as many nuclei are traversed, as are cells, then

$\frac{0.0366}{4} = 0.00915$ is fraction of nuclei traversed

Consideration of the Bragg ionization means ½ the nuclei traversed receive 2x the energy received by the other half.

Therefore, 0.00458 of nuclei receive 2x the dose of the remaining 0.00458 of nuclei (Call the doses x and 2x)

Let us now calculate dose to nuclei, for 1 Rad to the cells

$$(0.00458)(X)+2(0.00458)(X)=1.0$$

$$0.01374X=1.0$$

$$X=72.8 \text{ Rads}$$

$$\text{Therefore } 2x=145.6 \text{ rads.}$$

Therefore, for 1 Rad of 5 MEV alpha particles delivered, we have the following population distribution:

0.00458 of nuclei receive 145.6 rads
0.00458 of nuclei receive 72.8 rads
0.99084 of nuclei receive 0 rads

Now, we can calculate effective aging for the distribution, assuming delivery of radiation in the 8th year of life. Gofman and Tamplin have used (their Figure 1a) 1 Rad = 0.29 years.

So, 0.00458 of nuclei are aged (145.6) (0.29) = 42.2 years
0.00458 of nuclei are aged (72.8) (0.29) = 21.1 years
0.99084 of nuclei are not aged at all.

For the low LET radiation,
we have, for 1 Rad, 6.25×10^7 1 MEV β-particles
Range = 4000 microns
Cell diameter, for $523.7\mu^3$ volume, in the cubical array will be 8.05 microns

$$\text{Therefore } \frac{4000}{8.05} \cong 500 \text{ cells traversed}$$

$$\text{So } (6.25\times10^7)(5\times10^2) = 3.125\times10^{10} \text{ cells traversed}$$

But ¼ as many nuclei are traversed,
So 7.81×10^9 nuclei are traversed

$$\text{But there are } \frac{10^{12}}{523.7} = 1.91\times10^9 \text{ cells, or nuclei}$$

Therefore $\frac{7.81\times10^9}{1.91\times10^9}\cong4.1$ times, each nucleus is traversed

So, the low LET radiation can be considered approximately uniform. And, aging therefore $=1\times0.29=0.29$ years

We can now calculate the expected cancer incidence, as did Gofman and Tamplin for chronological age of 40 years.

	Effective age (years)	Fraction of nuclei	Cancer rate cases/10^6/year	Expected cancers (fraction×rate)
5 MEV α particles (1 rad)	82.2	0.00458	15,500	70.99
	61.1	.00458	4,950	22.67
	40	.99084	550	544.96
Total				638.62
Low LET radiation (1 rad)	40.29	1.0000	570	570
Spontaneous	40	1.0000	550	550

Therefore, for 5 MEV α particles (High LET), excess cancers=88.62 for 1 MEV β particles (Low LET), Excess cancers=20

$$R.B.E.=\frac{88.62}{20}=4.43$$

Conclusion

It appears that if nuclei represent the relevant parameter, the R.B.E. is much higher than the case estimated by Gofman and Tamplin for whole cells as the relevant parameter. More detailed calculations, at various dose levels, cell sizes, and particle energies will be worthwhile.

References

(1) Gofman, J. W., and Tamplin, A. R. "The Mechanism of Radiation Carcinogenesis." Testimony presented at hearings of the Joint Committee on Atomic Energy, 91st Congress, Part II, January 28, 1970 (GT-109-70).

APPENDIX IV

Questions by Senator Muskie and Replies by Witnesses

Contents:

(The materials below consist of questions asked by the subcommittee of several witnesses and their subsequent answers:)

QUESTIONS

Underground Uses of Nuclear Energy

1. Radioactive products from the underground uses of nuclear energy may be categorized as (1) fission products; (2) residual fissionable or fusionable materials; (3) thermonuclear-reaction products; (4) induced activities from device materials; and (5) induced activities from surrounding materials such as rock.

Could you furnish the Committee with an indication of the radioactive products of public health or environmental significance including a brief description of the criteria governing their selection, their relative abundance, and their natural occurrence in the environment?

2. Are there potential environmental effects from the civilian use of nuclear explosives?

a. What limitations are there to extrapolating past experience on the environmental effects of *contained underground nuclear explosive* experiments to proposed natural resource applications?

b. Gas stimulation is a planned use of nuclear explosives. What potential risks or hazards from radioactive materials (e.g. tritium, carbon-14, etc.) may accompany the introduction of the gas and petroleum products into interstate commerce?

c. What limitations are there to extrapolating past experience on the environmental effects of underground or atmospheric weapons testing to the effects of the civilian uses of nuclear explosives on aquatic and marine environments?

3. During the hearings, Dr. Robert Platt discussed ecological research efforts in connection with the Palanquin (April 1965) and Cabriolet (December 1968) Plowshare experiments. At this time he said:

> ". . . . it appears that the beta component of the fallout radiation is the effective radiation, and also that it may have a higher relative biological efficiency than does gamma, at least under the circumstances tested.
>
> "The beta dose from the fallout of these events is 4 to 12 times higher than the gamma and as such becomes the critical factor. This is the first time the beta component has been measured. All previous estimates of damage from fallout have been based on the gamma component alone, as for example predictions on the ecological effects of nuclear war."

To what extent do these observations suggest the need for a re-evaluation with respect to organisms that are not adequately shielded from beta radiation as are mammals by their thick skin? For example, plants are protected only by an epidermis, invertebrates have little protection, and micro-organisms have even less?

4. During the hearings Dr. C. Comar made the following general comments:

> "The important thing about fallout is that we do have experimental data so that there is some degree of confidence in our ability to estimate radiation exposures to man and the biosphere from given types of releases. A main difference is that worldwide fallout is dispersed over the entire earth whereas peacetime uses for the most part produce localized contamination. This means that specific attention has to be given to local conditions. Also, attention has to be given to some radionuclides that were not important in fallout.
>
> "Some data, but little compared to fallout, are available in connection with underground uses of nuclear explosives."

In his statement Dr. R. Platt said:

> "Most of the data and concepts on the irradiation of ecosystems have come from the use of experimental radiation fields using a point source of gamma radiation usually cesium–137 and cobalt–60.
>
> ". . . studies of the effects of beta radiation as a potential cause of plant damage has not been investigated previously in fallout fields. However, the hazard of beta radiation in fallout to skin has been recognized since the first nuclear detonation at Alamogordo."
>
> ". . . information needed on radiation effects. Areas of particular concern include: (1) sublethal effects from radiation does of less than 2–300 Rads, (2) effects of beta radiation to complement the extensive work done with gamma radiation, (3) subsurface effects and (4) effects extended in much greater depth in physiological processes or organisms as well as on the dynamics of ecosystems. These studies contribute directly to man by providing a better understanding of basic radiation effects on his environment, as for example productivity and stability."

In his statement Dr. Comar also stated:

> "Although a great deal is known about environmental radiocontamination much more needs to be done to provide a factual basis for the above assessments. The areas in particular need of research include: (1) low level radiation effects on biological systems including man, (2) evaluation of radiation dosages to be received by critical parts of the biosphere from planned technical operation, (3) similar evaluation of non-radiation effects, (4) meaningful studies of risk-benefit relationships."

Witnesses discussed current knowledge and research needs on the environmental and public health effects of radioactivity. What would you consider a realistic approach to the development of nuclear explosives for civilian application to insure an ability to evaluate the attendent environmental and public health risks?

5. Testimony indicates a difference of opinion in interpreting the available medical and scientific evidence on the hazards of tritium. Would you please comment on the following statements regarding [illegible]

a. Testimony of Dr. E. P. Radford, M.D. before the Committee on Public Works, Subcommittee on Air and Water Pollution, November 19, 1969, p. 3:

> "The principal health problem from tritium in small amounts appears to be from incorporation in the genetic material of the germ cells, particularly the ovaries of females. In this case, tritium laid down in the DNA of ova may remain for the reproductive life span."

b. Testimony of Dr. L. C. Cole before the Committee on Public Works, Subcommittee on Air and Water Pollution, November 20, 1969, p. 4:

> "However, tritium becomes a constituent of radioactive water and goes everywhere that ordinary water goes including into and through the bodies of all living plants and animals. It gets built into their organic compounds including the nucleic acids which carry the genetic information for the next generation. There is recent evidence that the weak beta particle from tritium may represent a greater biological hazard than much more energetic gamma radiation (reference, Huver, C.W., 1969, *Biological effects of tritium.* Mimeo., presented to the Minnesota Pollution Control Agency, February 19, 1969)."

c. Testimony of Dr. C. L. Comar before the Committee on Public Works, Subcommittee on Air and Water Pollution, November 19, 1969, p. 6:

> "We need more information about the role of tritium in biochemically important compounds with low turnover, about the significance of radionuclides such as Fe–55 that become incorporated into storage proteins, the relative importance of liver accumulation of plutonium as compared to lung deposition."

d. Statement for the Record of Dr. C. E. Larson before the Joint Committee on Atomic Energy, October 30, 1969, p. 25:

> "The energy characteristics of tritium and the kinetics of its origin, distribution, dilution and final disappearance are such as to eliminate the sudden emergence of tritium as a radiation hazard."

6. Dr. LaMont C. Cole in his testimony stated that ". . . in the Soviet Union the maximum permissible concentration of tritium in drinking water is only one-tenth of that permitted in this country." What are the medical, scientific, or public policy considerations that resulted in the selection of the Soviet Union's standard for tritium?

7. There is a recognized need for the pre-evaluation of social benefits and costs associated with the underground uses of nuclear energy. For civilian applications do you feel the Atomic Energy Commission, which is promoting the technology, is the appropriate body to make these decisions?

8. During 1966 hearings on radioactive water pollution in the Colorado River Basin, Dr. Peter Morris of the Atomic Energy Commission stated:

> ". . . we must look at each specific case to realize what we can do to minimize radiation. In some cases background levels exceed these guides, as he has mentioned, for example, in some of the drinking water wells used by millions of people in the Midwest. In other cases background levels, for example, in the Colorado River now, are way below background levels. We must ask our-

selves, is the effort to reduce the levels which we can control worth the gain?"

Do you feel this attitude is reflected in policies of the Atomic Energy Commission or the Federal Radiation Council, and do you feel current procedures for evaluating the social worth of reducing environmental radiation levels is appropriately made by these organizations under existing procedures?

9. On May 8, 1969, John S. Kelly of the Atomic Energy Commission commented before the Joint Committee on Atomic Energy of the relative roles of Government and the user in conducting a commercial explosive service, "We hope eventually to limit the Government's role in these matters to a review and inspection function to assure that the guide lines are met. By doing so, we will be relying on industry's ingenuity and skill to develop simplified and efficient methods of operation."

Although guidelines will be developed, to what extent can or should industry be relied upon to evaluate the potential environmental effects or monitor the attendant effects of the commercial uses of nuclear explosives?

10. In the past the Atomic Energy Commission has been accused of being too restrictive with information on the environmental effects of its nuclear tests. What information do you feel should be made available that is currently restricted?

11. What would be an effective policy regarding the roles of various Federal, State, and local government agencies in evaluating the potential environmental risks and precautions in applications of this technology?

12. Would you comment on the following statements in relationship to current medical and scientific evidence on the public health and environmental effects of radiation?

On June 29, 1965, Dr. Paul Tompkins, Executive Secretary, Federal Radiation Council stated:

> "The numerical values for these Guides were placed as close to the annual dose from natural background radiation as technical, economic, and operational considerations in the nuclear industry allowed."

On November 16, 1969, he said:

> ". . . increasing knowledge over the past 15 years indicates that the radiation dose prescribed by the FRC is probably less rather than more hazardous to health and well-being than was thought at the time."

13. The standards adopted by Federal and international agencies concerned with radiation protection to the population at large are based on the idea of a maximum permissible average dose to the body tissues. Would you comment on this concept as it relates to the following statement by Dr. Edward P. Radford?

> "The concept of an average physical dose to a tissue or to an organ is not necessarily meaningful when applied to effects on individual macromolecules in cells or when the radiation source is sharply localized, as for a radioactive particle of about 1 micron in diameter. The question of the effective radiation exposure to a macromolecule is important when the genetic changes in the DNA of the germ cells are considered."

REPLY FROM DRS. TAMPLIN AND GOFMAN

UNIVERSITY OF CALIFORNIA,
LAWRENCE RADIATION LABORATORY,
Livermore, Calif., February 9, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, U.S. Senate, Committe on Public Works, Washington, D.C.

DEAR SENATOR MUSKIE: We are pleased to have the opportunity to reply t several of the questions in your draft concerning underground uses of nuclea energy. A number of the questions are definitely within our area of specialt and these are answered in the attached material, which, of course, you may us in any way that is helpful.

In a general way, however, we should like to state our opinion of the need fo underground nuclear explosions, aside from the specific questions.

We have just about as much need right now for holes created underground fo any reason, by peaceful nuclear explosives as we have for holes in our head above ground.

With kindest regards.

Sincerely,

ARTHUR R. TAMPLIN.
JOHN W. GOFMAN.

P.S. Every answer provided in the question-answer section is in *absolute dea earnest*. We are in no sense being facetious on any one of the questions.

P.P.S. We are also enclosing some correspondence related to Dr. M. King Hul bert's presentation at the University of Minnesota Symposium on Nuclear Pow and the Public. He refers to a 92 page report, dated May 1966, that was pr pared by the National Academy of Sciences, that the *A.E.C. has steadfastly r fused* to release to the public. This is a very specific case that dramatically an swers your questions 7, 8, 9 and particularly 10. Dr. Hubbert is an eminen highly respected Scientist in the field of Natural Resources, so when *he* refers t A.E.C. "cosmetizing" of reports, everyone interested in staying alive shoul listen carefully to Dr. Hubbert.

(NAS Report and other relevant material appears in appendix V.

ANSWERS TO QUESTIONS—UNDERGROUND USES OF NUCLEAR ENERGY

Question 1. Which radioactive products are of public health or environment significance?

Answer: Our work indicates that all radioactivity is harmful both to man an to members of the ecosystem that supports him. No evidence for any sort of saf threshold exists and furthermore we have recently shown that no protection a all should be anticipated from slow versus rapid delivery of radiation. Thu any or all radioactivities that gain access above ground can do harm to ma plants, and animals. And for those that remain below ground, sooner or late some of these are going to gain access to underground waters and finally end u doing harm to man and his ecosystem in remote locations from the undergroun explosion.

Perhaps the best answer concerning which radioactivities are of public healt significance might be to say that there is little choice between death by arseni poisoning or by cyanide.

Question 2. Are there potential environmental effects from the civilian uses o nuclear explosives?

Answer. Yes, disastrous ones. Since every amount of radioactivity is harmfu the harm will be in proportion to the extent civilian nuclear explosives are use **A little is bad enough; a lot is much worse.**

2a) What limitations are there to extrapolating past experience on environmental effects of "contained underground nuclear" explosive experiments to proposed natural resource applications?

Answer: There is no limitation. Everything we know from past experience tells us such applications make no sense at all. First of all, the term "contained" is a misnomer. No one is going to do *contained* explosions unless some natural resource is *taken out*. And essentially *every possible* application means taking out a product that is radioactive—and that simply means death and harm to man and his ecosystem. Let us not fool ourselves about this deceptive word, "contained".

2b) What hazard may accompany the introduction of gas and petroleum products into interstate commerce?

Answer: We will produce genetic damage, human misery, cancer, and leukemia in this way in direct production to the extent that such gas finds access to humans, which of course it will.

The proponents tell us to think of the benefits versus the risk. We have done so. The people will *buy* the gas and get cancer and die; the gas and petroleum companies will get increased profits selling the poison gas. This is called weighing the benefits versus the risks.

2c) Consideration of past experience concerning aquatic and marine environments?

Answer: We know plenty from past experience to tell us that fish and other marine animals will concentrate many radioactivities 100's, 1000's or more times. As a result we will make our marine food sources a potent source of radioactive poison for humans.

Question 3. Dr. Robert Platt—and the difference between betas and gammas?

Answer: This is the sheerest nonsense, totally devoid of any scientific content whatever. Everyone who understands the elementary nuclear physics realizes that gamma rays only produce effects upon material (animate or inanimate) to the extent that they are converted either by photoelectric or compton effect into electrons—and electrons *are* beta rays. Years and years ago this was established to everyone's satisfaction. Beta rays and gammas are equivalent if they deposit their energy in tissue. Dr. Platt's statements represent pure, unvarnished rubbish, based upon poor data or none.

Question 4. What would you consider a realistic approach to the development of nuclear explosives for civilian application to insure an ability to evaluate the attendant environmental and public health risks?

Answer: (a) The only really sound approach is immediate complete cancellation of this entire idiotic technology, and to put the funds and effort into some constructive activity.

(b) Assuming we insist on the folly of continuing this lunacy, the best thing to do would be to keep *all* promoters of the technology, individuals, companies, or governmental agencies *completely* divorced from anything to do with evaluation of environmental and public health risks?

Question 5. On tritium.

Answer: To the extent that tritium gains access to the biosphere it will produce, per unit of energy dissipation in tissue, as much harm, and *possibly*, but not definitely, a little more harm than, other radionuclides. All radionuclides, tritium included, are harmful. For example, in natural gas stimulation, tritium will be a major contributor to producing the extra cancers and genetic deaths. One is just as dead from cancer produced by tritium as from any of 700 other radioactivities, or from an X-ray machine.

Question 6. What are the medical, scientific, or public policy considerations that resulted in the selection of the Soviet Union's standard for tritium?

Answer: The Soviet Union does many very stupid and evil things in many areas. We can't find much favorable to say about the Soviet Union. However, in regard to tritium standards, we would say that their standard setting bodies would appear to be approximately 1/10 as stupid as ours.

Question 7. For civilian applications do you feel the Atomic Energy Commission, which is promoting the technology, is the appropriate body to make these decisions?

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Answer: We cannot think of *any* possible agency *less* appropriate to make these decisions than the Atomic Energy Commission. Abundant past experience should long ago have taught us this.

Question 8. Is the effort to reduce levels worth the gain?

Answer: Too many people are ready to sell human lives cheaply. We feel the policies of the Atomic Energy Commission and the Federal Radiation Council in this area represent nothing short of a national disgrace. Both agencies are perfectly willing to play Russian Roulette with the lives of individuals and the future of life on earth. In every area where uncertainty exists, both agencies always decide in favor of technology, and against the public health and welfare. That is all too obvious from everything that has occurred.

Question 9. To what extent can or should industry be relied upon to evaluate the potential environmental effects or monitor the attendant effects of the commercial use of nuclear explosives?

Answer. If our objective is to end life on earth, we can place great reliance on industry in these areas. If we have *any* other objective, we should see to it that industry have *nothing* whatever to do either with evaluation *or* monitoring.

Question 10. What information (from A.E.C.) do you feel should be made available that is currently restricted?

Answer. It is necessary to get the Nevada Operations Office out of funding, and effectively controlling, the Southwestern Regional Laboratory of the U.S. Public Health Service. Until we do, we'll never even know what information is being restricted. The A.E.C. should have *zero* control over what any monitoring body does. Further, they should not be allowed to withhold information concerning certain radionuclides, released upon the public by hiding behind a National Security Classification screen.

(See also the P.S. in our letter and the attached correspondence relating to Mr. King Hubbert's presentation at Minnesota).

Question 11. What effective policy regarding the roles of various Federal, State, and local agencies in evaluating potential environmental risks in this technology?

Answer. The only *really* sensible approach is to abolish this ridiculous technology now.

If we refuse to be this sensible, then State and local agencies should have the sovereign privilege of being *more* restrictive than the Federal agencies.

Furthermore, when citizens protest being poisoned and victimized by such idiotic technologies, they should be provided with equal funds and legal assistance to match the entire Solicitor General's office which is at the beck and call of the A.E.C.

Currently the A.E.C. can spend millions of taxpayer dollars to fight off any citizen who objects to being poisoned. Only a multi-millionaire can afford to object to some of the hare-brained projects of the A.E.C.

Question 12. Concerning Dr. Paul Tompkins statements concerning the F.R.C. Guidelines.

Answer. Dr. Tompkins statements are false, incorrect, wrong, and ridiculous concerning hazard. He is indeed correct in saying they are placed as low as is convenient for the nuclear industry. In essence he is saying that nuclear industry decides what is *conveniently* possible for them, and then the public must accept the attendant deaths and misery. Stated otherwise, technology above all, humans be damned.

Question 13. Concerning the maximum permissible average dose to tissue versus Dr. Radford's comments on a sharply localized source?

Answer. Maximum permissible dosage is a nonsense concept from the outset. It is defined better as "How many cancers or genetic deaths are acceptable?" There is no dosage without harm, so the proper permissible dose is zero, unless we are going to suffer a worse fate by *not* being irradiated.

If we *must* be irradiated, Dr. Radford *is* correct that localized irradiation may very well prove much more damaging than diffuse irradiation. We recently sent you an article on this entitled "Mechanism of Radiation Carcinogenesis" by J. W. Gofman and Arthur R. Tamplin.

The only *conceivable* basis for going ahead with any of this underground nuclear explosive technology would be that the fate of humans will be worse if we don't go ahead than if we do. Much more cogent evidence will be required to make any rational humans believe we have reached this point.

Dr. Edward Teller, a fervent proponent of nuclear explosives above ground, underground, and on the moon has stated we won't know the potential benefits unless we go ahead with the technology. In answer we can only say this sounds like advice to just point your gun anywhere and shoot. Who knows, you may bag a duck!

UNIVERSITY OF MINNESOTA,
Minneapolis, Minn., January 7, 1970.

Dr. M. KING HUBBERT,
Research Geophysicist, U.S. Department of the Interior, Geological Survey, Washington, D.C.

DEAR KING: In putting the last touches on the typescript of our proceedings prior to submission to the publisher, we have been faced with a problem of propriety with respect to your paper.

The insert (pages 36a and 36b) contain allegations concerning action or, rather lack of action on the part of the AEC that were not made openly at the meeting sessions, at which time the AEC might have replied had they chosen to do so. This being the case, we have made an editorial decision to delete the last two paragraphs of the insert as is indicated on the copy we are sending you. We hope you understand why this is necessary.

Best regards.

Sincerely yours,

HARRY FOREMAN, M.D., *Director.*

HUBBERT, "INDUSTRIAL ENERGY RESOURCES"

Hubbert, "Industrial Energy Resources," is to monitor all waste-disposal practices, and to make *public* its reports of its activities and findings.

In view of the public concern over the question of environmental contamination by atomic wastes from nuclear reactors manifested at this meeting, it is pertinent to state that during 1965, at the request of the Atomic Energy Commission, the Committee on Geologic Aspects of Radioactive Waste Disposal of the Division of Earth Sciences, National Academy of Sciences—National Research Council, made a final review of the waste-disposal practices at the following establishments of the Atomic Energy Commission:

1. Savannah River Laboratory, South Carolina
2. Oak Ridge National Laboratory, Oak Ridge, Tennessee
3. Carey Salt Company mines, Hutchinson and Lyons, Kansas, to witness storage of high-level wastes in accordance with the same committee's earlier recommendations
4. National Reactor Test Station, Arco, Idaho
5. Hanford Atomic Products Operation on the Columbia River in Washington

The report of this committee, dated May 1966 and comprising 92 pages, was submitted to the Atomic Energy Commission. Notwithstanding the fact that all earlier reports of this committee had been released to the public, and despite repeated requests from two successive chairmen of the Earth Sciences Division, release of this report by the Atomic Energy Commission has been persistently refused.

This represents a particular instance—one of my personal acquaintance—of the AEC practice, described so effectively by Dr. Harold P. Green in his paper before this symposium, of withholding from the public, or "sweeping under the rug," information that is unfavorable to the "cosmetising" treatment to which most AEC documents are subjected. While only the Atomic Energy Commission has the authority to release this report, it appears that the time has come when a concerned public, as well as Chairman John E. Moss of the Government Operations Subcommittee on Government Information, should at least become aware of its existence.

1128152

U.S. GEOLOGICAL SURVEY,
Washington, D.C., January 21, 1970.

Dean E. ABRAHAMSON, M.D., Ph. D.,
Medical School, Department of Anatomy,
Minneapolis, Minn.

DEAR DR. ABRAHAMSON: Thank you very much for sending me a copy of your letter of January 19, 1970, to Dr. Harry Foreman regarding the exclusion of the insertions, pages 36a and 36b, to my text, for the symposium on Nuclear Power and the Public, prepared prior to the meeting. I am attaching a copy of my handwritten reply of January 13 to Dr. Foreman's letter to me of January 7.

As I see the picture, we are involved in problems of two different kinds: (1) a question of editorial policy and consistency, and (2) a question of the relevance of the information contained in the insertions to the purposes of the symposium and the interests and concerns of the audience and potential readers.

With regard to the first question, I defer completely to the judgment of the members of the Program Committee. Concerning the second, it is my opinion that the information pertaining to the review of AEC waste-disposal practices made by an eminently qualified committee under the auspices of the Division of Earth Sciences, National Academy of Sciences—National Research Council, and of the AEC's subsequent refusal to permit the report (which contained no classified information) to be made public, is highly relevant to the purposes of the Minnesota symposium. So long as the public remains unaware of the existence of this report, the AEC can keep it buried, but probably not for very long afterwards.

In preparing my written paper prior to the meeting, I was largely in the dark concerning what the issues and purposes of the meeting were. During the meeting, I carried in my hand a copy of this waste-disposal report, hoping for a suitable opportunity to insert this information into the record, but either none came, or else I missed it.

In view of the importance of the information conveyed by my proposed insertions, I should appreciate having the Program Committee reconsider their acceptability, in accordance with the suggestion of the last sentence of your letter.

Sincerely yours,

M. KING HUBBERT.

U.S. GEOLOGICAL SURVEY,
Washington, D.C., January 13, 1970.

HARRY FORMAN, M.D.,
University of Minnesota, Mayo Memorial Building,
Minneapolis, Minn.

DEAR HARRY: In reply to your letter of January 7, if the last two paragraphs of my insert on pages 36a and 36b are to be deleted; the first must also be. Without the last two paragraphs, the remainder of the insertion is pointless.

I regret that my advance information concerning the most important issues at this conference was inadequate to permit me to deal with this important aspect effectively.

Yours sincerely,

KING.

UNIVERSITY OF MINNESOTA,
Minneapolis, Minn., January 20, 1970.

DEAN E. ABRAHAMSON, M.D., Ph.D.,
Department of Anatomy,
Medical School,

DEAR DEAN: Thanks for your letter of January 19. As I had already told you, at King's request, we are omitting the whole insert.

Incidentally, the editorial revisions by others that were accepted contained no significant changes in substances in contrast to King's requested insert. In our instructions to the participants we asked that there be no additional conceptual material added and all, except King, complied. If you found substantive changes, please let me know.

Sincerely yours,

HARRY FOREMAN, M.D.,
Director.

REPLY FROM PROF. MARVIN KALKSTEIN

STATE UNIVERSITY OF NEW YORK,
STONY BROOK, LONG ISLAND, N.Y., *February 17, 1970.*

HON. EDMUND S. MUSKIE,
U.S. Senate.
Chairman, Subcommittee on Air and Water Pollution,
Washington, D.C.

DEAR SENATOR MUSKIE: I am sorry to be so late in responding to your letter of January 23; I hope that my answers to the questions you posed will still be of use. I have not had the opportunity to look into many of these questions in much detail, but I will attempt to answer them as fully as I can and to at least convey my own general impressions about them.

Question 1. Which radioactive products are of public health or environmental significance?

1. A listing of radioactive products of public health or environmental significance is based upon a number of criteria: these would include the amounts produced in a nuclear explosion; the types of radiation they give off; the duration of exposure to these radiations and their biological significance in terms of entry into biological systems. Attached is a table indicating a number of radioactive products; their half lives; their means of production according to the categories listed in your first paragraph of your draft questions on underground uses of nuclear energy: yields per megaton (for fission products based on slow-neutron fission of uranium-235; hydrogen-3, carbon-14, and argon-39 produced by activation in air; sodium-24 to zinc-65 produced by activation in soil and in device materials; plutonium-239 would be as a residual fissionable material) for some of these radionuclides the table includes indication of biological significance in terms of the sites of concentration within the body, means of transport or attachment and significant radiations. This table is based on other tables and materials that are a few years old, but should be sufficientiy accurate for your purposes.

Particularly significant fission products include strontium-90, strontium-89, cesium-137 and iodine-131 and zirconium-niobium-95. The strontium-90 and 89, cesium-137 and iodine-131 are significant internal emitters. The strontium products are bone-seekers and the iodine-131 concentrates in the thyroid. Cesium-137, which emits an energetic gamma ray, can have a genetic effect.

A recent report by Ernest Sternglass (testimony presented at New York hearings held by Congressmen Addabbo, Reid, and Wolff on Atomic Energy in the Environment, February 6, 1970), while still open to question, suggests that yttrium-90, the daughter product of strontium-90, may concentrate in the gonads or other sensitive tissue causing genetic or somatic effects.

Zirconium-niobium-95 decay with about a 60-day half-life and are in very high abundance shortly after a nuclear explosion and constitute a major part of the external radiation field for the first half-year subsequent to a nuclear explosion. Cesium-137 can also be listed among the external emitters contributing to radiation dosage from exposure to external sources. Of the isotopes listed in the table only hydrogen-3 and carbon-14 occur naturally in the environment. During periods of peak atmospheric nuclear testing in the late 50's and in 1961–2, these were produced artificially at a high rate compared to their natural production.

Question 2. Are there potential environmental effects from the civilian uses of nuclear explosives?

2. There are a number of potential environmental effects in the civilian uses of nuclear explosives. For large underground nuclear explosions, there is the possibility of generating earthquakes; and in the case of explosions in coastal regions, the possibility exists for the generation of tsunamis (tidal waves). Ground shock from large underground explosions can also cause damage at and near the surface in a sizeable region around the site of the explosion. Where the explosions take place close to the surface, as in cratering experiments or excavations, damage

can be caused by air blast. For shallow explosions or for any explosions in which there may be the release of radioactive products, there may be environmental effects due to the radiation from those products.

2a) What limitations are there to extrapolating past experience on environmental effects of "contained underground nuclear" explosive experiments to proposed natural resource applications?

(*a*) Extrapolations of past experience and the environmental effects of continued nuclear explosive experiments are limited in that while one can use scaling relationships between the depth and energy of the explosion, one must be careful to take into account the media in which the explosion takes place. Most of our experience with volcanic tuff in Nevada and the results from these experiments may be of only limited use in applications under different geological conditions. Furthermore, particularly with respect to the question of venting of radioactive debris, one must be concerned about anomalous occurrences which one cannot predict on the basis of experience.

2b) What hazard may accompany the introduction of gas and petroleum products into interstate commerce?

(*b*) The potential of risk from radioactive materials in natural gas whose flow was stimulated by the use of nuclear explosives is real. Results from the first such experiment, Project Gasbuggy, indicated that the natural gas released by this test was contaminated with so much tritium that it was not saleable (*Washington Post*, 11/13/68, page 84).

2c) Consideration of past experience concerning aquatic and marine environments?

(*c*) Again, with reference to the effects of civilian uses of nuclear explosives on aquatic and marine environments, I think our past experience from underground or atmospheric weapons testing will be of limited value. These tests were conducted in limited areas and results, therefore, may only pertain to a limited number of aquatic or marine species. The conditions of proposed civilian uses may also be sufficiently different so that one would not be able to directly relate the seismic, pressure wave, and radiation effects from past weapons tests.

(Question 3 omitted.)

Question 4. What would you consider a realistic approach to the development of nuclear explosives for civilian application to insure an ability to evaluate the attendant environmental and public health risks?

4. Although I am not in a position to judge how realistic it would be from a political point of view, I would consider it reasonable to have a moratorium on the development of nuclear explosives for civilian application until we are better able to evaluate the attendant environmental and public health risks. I agree with Doctors Platt and Comar on the need for further information on the low-level radiation effects on biological systems and for a more meaningful study of the risk-benefit relationships. While there is a considerable volume of information relating biological effects to dose at high dose rates and studying the effects of low doses on animals, there has as yet been very little epidemiological studies of the effects of low doses on man. I would guess that we may be at least 5–10 years away from any significant answers in this area and so would regard a moratorium on Plowshare of 5 or 10 years as being reasonable. It may be that certain deep underground applications are safe enough to be allowed, but we certainly should not be proceeding further with cratering experiments at this moment. The benefits are far from established for the use of nuclear explosives rather than more conventional techniques for the various applications being considered. It is likely that there may be no gain in using nuclear explosions in some cases. We could stand further study of alternate methods of pursuing these objectives before proceeding with the nuclear explosions. For this reason also a moratorium would be useful.

Question 5. On tritium.

5. I think the statements by Doctors Radford, Cole, and Comar raise enough question with regard to the hazards of tritium that it cannot be easily dismissed on the basis of Dr. Larson's statement. I think further investigation of the biological significance of tritium would be in order.

(Question 6 not answered.)

Question 7. For civilian applications do you feel the Atomic Energy Commission, which is promoting the technology, is the appropriate body to make these decisions?

7. I think that the Atomic Energy Commission is not the appropriate body to make decisions with regard to the social benefits and costs associated with the underground uses of nuclear energy. As indicated in your question, the AEC is the agency that has been promoting the technique. The record of the AEC thus far gives me no confidence that they would be able to fairly perform both functions at the same time. I think the AEC is a prime example of a government bureaucracy that has been so carried away by its sense of mission that it has lost sight of its role being one to serve the public interest.

Question 8. Is the effort to reduce levels worth the gain?

8. I feel that the current procedures for evaluating the social worth of reducing environmental radiation levels as carried out by the AEC or the Federal Radiation Council is inappropriate for the protection of the public. The tendency as indicated in some of their guidelines and expressed by some of their spokesmen to compare the hazards of extra radiation with that from background levels only serves to beg the question of appropriate radiation standards and to some extent obscures an important aspect of the problem. We should be asking about the costs to whom and the gain by whom. In introducing extra radiation into the environment, we are in the position of having an effect upon the individual with no choice allowed to that individual to whether he or she wishes to be so effected. Most of the comparisons that have been made for radiation effects have either been with respect to background where there is essentially no choice for anyone in the matter or with respect to such hazards as smoking, driving, etc., where the individual does have some element of personal choice.

Question 9. To what extent can or should industry be relied upon to evaluate the potential environmental effects or monitor the attendant effects of the commercial use of nuclear explosives?

9. I don't think we can go far toward relying upon industry to evaluate potential environmental effects or to monitor the attendant effects of the commercial uses of nuclear explosives. I think the evaluations of the potential effects are difficult and complex and will depend to some degree upon the perspective of those doing the evaluation. At the very least most industries would need some outside help in order to make a thorough evaluation in any case. Of course, it is possible for an industry to monitor the effects of its own activity, but in order to assure the reliability of such results, it would be desirable to have an independent monitoring of these activities.

Question 10. What information (from A.E.C.) do you feel should be made available that is currently restricted?

10. I am not particularly well informed as to what information is currently restricted or not. I would guess the problem would be one less of restriction because of the confidential or secret nature of information and more a problem with regard to the ease or difficulty of access to such information. In any case, I would feel that except for device design or other information of clearly military significance, that all information with regard to the environmental effects of nuclear tests and their applications should be made readily available.

Question 11. What effective policy regarding the roles of various Federal, State, and local agencies in evaluating potential environmental risks in this technology?

11. I think that federal and state government agencies should provide data and assist in evaluating the potential environmental risks and precautions in applications of nuclear technology but the decisions with regard to whether the applications should be carried out and under what conditions or standards should be made at the local level.

Question 12. Concerning Dr. Paul Tompkins' statements concerning the F.R.C. Guidelines?

12. I am not sure where we stand now as compared to 15 years ago with regard to the degree of hazard that we face from a given radiation dose; but as I have indicated earlier, I feel that background radiation is really not an appropriate criterion for establishing dose limits from nuclear sources. Furthermore, I feel that considerations other than technology, economics, and operational should be foremost in deciding what to allow nuclear industry. These should obviously include the public health but should also include an assessment of the real public need for the activities and results generated by these industries.

Question 13. Concerning the maximum permissible average dose to tissue versus Dr. Radford's comments on a sharply localized source?

13. I think that Dr. Radford's point is well taken. Apparently, there is basis for real question beyond the genetic effects with regard to doses to particular organs. Furthermore, there is reason to seriously question anew the role of internal emitters. As mentioned earlier, the data of Sternglass, if correct, suggest that certain internal emitters may have large effects even at relatively low doses. Without judging the correctness of his assertions, they are serious enough to warrant further careful investigation.

Sincerely yours,

MARVIN KALKSTEIN.

Enclosure.

RADIOACTIVE PRODUCTS OF PUBLIC HEALTH OR ENVIRONMENTAL SIGNIFICANCE

	Half-life	Yield, C/MT	Means of production	Biological significance
H^{3}	12.3 years	<1	(2), (3), (5)	Water organic material.
C^{14}	5,600 years	3.4×10^{4}	(5)	Organic material.
Na^{24}	15 hours	2.8×10^{11}	(5)	
P^{32}	14 days	1.92×10^{6}	(5)	Do.
A^{39}	~260 years	59	(5)	
K^{42}	12 hours	3×10^{10}	(5)	(Natural K^{40}) muscle.
Ca^{45}	152 days	4.7×10^{7}	(5)	Bone.
Fe^{55}	2.9 years	1.7×10^{7}	(4), (5)	
Fe^{59}	46 days	2.2×10^{6}	(4), (5)	
Co^{60}	5 years		(4), (5)	∂
Zn^{65}	245 days		(4), (5)	Concentration in fish.
Sr^{89}	51 days	1.7×10^{7}	(1)	Boneseeker.
Sr^{90}-Y^{90}	28 years	1.0×10^{5}	(1)	Do.
Zr^{95}-Nb^{95}	65.5 days	$\sim2\times10^{7}$	(1)	∂ external.
Ru^{106}-Rh^{106}	1 year		(1)	
I^{131}	9 days	7.3×10^{7}	(1)	Ehyroid.
Cs^{134}	2 years		(1)	
Cs^{137}	27 years	1.4×10^{8}	(1)	Genetic.
Ba^{140}-La^{140}	12.8 days	$\sim10^{8}$	(1)	Boneseekers.
Ce^{144}	282 days	$\sim5\times10^{4}$	(1)	
Pu^{239}	24,000 years	$3.6\times10^{[illegible]}$	(2)	∂ inhalation.

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REPLY FROM EDWARD P. RADFORD, M.D.

Question 1. Which radioactive products are of public health or environmental significance?

Answer to question 1. In my opinion the radioactive products of an underground detonation of public health significance are the same as those arising from weapons testing or from nuclear power development. These include iodine 131 and xenon 133, tritium and krypton 85, strontium 90 and cesium 137, and unexploded fissionable material. Short-lived isotopes such as iodine 131 and zenon 133 are significant because they may readily escape from an underground site if there is any significant breach of the surface. They are both volatile and can be carried by the wind in contact with human populations. Iodine enters the human food chain quite readily, and because iodine is taken up selectively in the thyroid it may have a concentrated effect there. Tritium and krypton 85 are important because of their long physical half-lives and the fact that they are probably the two most significant radionuclides to be produced by an underground explosion, in terms of the difficulty in containing them. Krypton produces its effects by irradiating the individual throughout the whole body, as does tritium, but tritium's effect on genetic material in the gonads may be especially important. Strontium 90 and cesium 137 are both long-lived radionuclides that can enter the water and come in contact with people. Because they may be taken up in the body similarly to calcium and potassium, both elements necessary for life, they may be selectively concentrated in certain areas of the food chain. Finally, plutonium is a particularly serious problem because of its great radiotoxicity if ingested or inhaled as a particle.

Except for tritium, none of these radionuclides are naturally present in the environment, and tritium is present only in extremely small amounts from cosmic radiation. As I pointed out in my testimony of Nov. 19, 1969, the amount of tritium that might be released from a thermonuclear detonation is many, many times greater than that from a fission bomb, and in fact is greater than all the long-lived radionuclides from a fission bomb put together.

Question 2. Are there potential environmental effects from the civilian uses of nuclear explosives?

2a) What limitations are there to extrapolating past experience on environmental effects of "contained underground nuclear" explosive experiments to proposed natural resource applications?

2b) What hazard may accompany the introduction of gas and petroleum products into interstate commerce?

2c) Consideration of past experience concerning aquatic and marine environments?

Answer to question 2. As far as potential environmental effects are concerned—(*a*) the limitation to extrapolating past experience is that the data from several of the previous Plowshare explosions are still being obtained, and thus the experience is not yet complete. For example, the issue of separating krypton-85 from the gases in the explosion site has not yet been settled, to my knowledge. Secondly, the extent to which tritium can be separated from the methane, ethane and other gases will also have to be determined. (*b*) With regard to tritiated gases being introduced into interstate commerce, obviously the amounts being delivered would have to be monitored in order to determine what potential risks might exist. It is apparent that a substantial amount of the tritium will be present in this form. (*c*) The environmental effects of underground or surface explosions will depend greatly on the local conditions, and thus previous data may not be applicable. From past experience, surface detonations probably will deliver a great deal of radioactivity to the atmosphere and thus carry the hazard to other countries around the world.

Question 3. Dr. Robert Platt—and the difference between betas and gammas?

Answer to question 3. It is not clear to me what was Dr. Platt's point in connection with the relative significance of beta and gamma radiation sources. It seems to me that the principal problem has to do with the way that radionuclides

of all kinds will move into locations where they may come in contact with people. In this case the evidence is still far from complete. Direct effects on plants ought to be fairly predictable.

Question 4. What would you consider a realistic approach to the development of nuclear explosives for civilian application to insure an ability to evaluate the attendant environmental and public health risks?

Answer to question 4. The answer to this question is similar to the one mentioned above in question 3. In my opinion a realistic approach to the problem of developing nuclear explosives for civilian application is:—(1.) Establishment and maintainence of environmental exposure levels (dose limits) sufficiently strict that the likelihood of any serious genetic implications of the distribution of these radionuclides will be small. (2.) A slow and careful use of experimental detonations in order to get beter answers to some of the questions raised by Drs. Platt and Comar. Finally, (3.) continuing research on the biological effects of low level radiation exposures. Unfortunately, this phase of research has not been popular in recent years, and with the cut-back in federal research funds there has been a reluctance to encourage and suppor research in this area.

Question 5. On tritium.

Answer to question 5. I believe the potential hazard of tritium is more serious than the testimony of Dr. Larson would indicate. If tritium is distributed from an underground detonation widely into the immediate vicinity, it will have the opportunity to move over the long distances. In the process it will of course be dimin-diluted by non-radioactive hydrogen, and to this extent its hazard will be diminished. However, studies to date have indicated that the biological organisms living in the vicinity of tritium will come into fairly rapid equilibrium with it, and if the particular region where the tirtium is distributed happens to be a significant source of food or water, then tritium is highly likely to enter the hydrogen pools of the human population consuming that food and water.

Question 6. What are the medical, scientific, or public policy considerations that resulted in the selection of the Soviet Union's standard for tritium?

Answer to question 6. I am not able to comment on the basis of the Soviet Union's standard for tritium release.

Question 7. For civilian applications do you feel the Atomic Energy Commission, which is promoting the technology, is the appropriate body to make these decisions?

Answer to question 7. I do not believe that the Atomic Energy Commission should be responsible for the final evaluation of social benefits and risks associated with underground uses of nuclear detonations. The whole question of the very complex interaction of environmental agents on the human population will, in my opinion, have to be assigned to a scientific advisory group of wide representation, responsible directly to the President or to the Congress, and whose decisions will be capable of periodic review and modification. This group should have the responsibility of drawing in experts from a wide variety of fields to provide the information necessary for making a final decision. An agency such as the Atomic Energy Commission, concerned only about radioactive hazards, is not in a position to evaluate many other environmental considerations about the use of nuclear explosions.

Question 8. Is the effort to reduce levels worth the gain?

Answer to question 8. The record today indicates that the Atomic Energy Commission has not evaluated the degree to which reduction of environmental radiation levels is feasible and worthwhile. They have indicated that as long as the acceptable MPC concentrations are not exceeded, this is an adequate basis for control. In my opinion the multiple kinds of environment agents that may produce mutations in man must be taken into account in evaluating the suitability of radiation exposures. Up to the present no attempt of this kind has been made on the part of the AEC or the Federal Radiation Council.

Question 9. To what extent can or should industry be relied upon to evaluate the potential environmental effects or monitor the attendant effects of the commercial use of nuclear explosives?

Answer to question 9. I have no doubt that industry would do its best to meet whatever regulations are set out in the guidelines, but I do not think that they would be inclined to question the guidelines themselves. In some cases the decision, for example, to use a thermonuclear bomb rather than a fission bomb might make a very significant difference in the total amount of tritium to which

the particular area might be exposed. In this case there could be an overlap between the use of a region for a fuel reprocessing plant and the decision to use an underground detonation which would release large amounts of tritium. The evidence is already clear that the broad overall considerations of environmental protection become lost by the industrial representatives in the calculations they carry out related to their own particular concerns.

Question 10. What information (from A.E.C.) do you feel should be made available that is currently restricted?

Answer to question 10. I am not aware that the AEC has been restrictive with information on the environmental effects of nuclear tests. On the other hand, I also believe that they have not been as vigorous in determining the environmental effects as they might have been. In any case I think that before further underground detonations are made, a simplified summary of the available information should be made to the Committee. At the present time it is difficult to obtain information because it is widely scattered throughout government and other reports and has not been brought up to date on the basis of the most recent information.

Question 11. What effective policy regarding the roles of various Federal, State, and local agencies in evaluating potential environmental risks in this technology?

Answer to question 11. I think the most effective policy regarding the roles of different governmental agencies is to recognize that they do in fact, each have a role. That is, if a local or state government elects to maintain a certain environmental quality for reasons of its own, such as for the maintainence of a recreational use of land or water, their opinions should be given weight, and the cost-benefit evaluation be done in the light of that decision. Up to the present time national interests seem to override those of local areas and this is a classical way in which a protest can be engendered from the citizens at large. The regulatory agencies concerned with defining the risks and benefits must be extremely careful that all information is presented in an impartial fashion, to avoid the charge that some considerations of public health have been ignored.

Question 12. Concerning Dr. Paul Tompkins' statements concerning the F.R.C. Guidelines?

Answer to question 12. I commented in my testimony of Nov. 19, 1969 that in my opinion, the present maximum permissible concentrations of radionuclides do not adequately take into account multiple sources of mutagenic agents in the environment. For this reason it is quite probable that the permissible concentrations of at least some radionuclides will have to be lowered since these are the ones cited above very likely to come in contact with human populations if peaceful use of nuclear detonations becomes widespread. I do not agree with Dr. Tompkins that the radiation guides have been placed as low as technical and operational considerations allow. Nor do I agree that the radiation dose permitted by the FRC is less hazardous to health than was thought at the time they were proposed. This is based on the idea of repair from genetic damage, and although there is some evidence to this effect, it is far from comforting in terms of establishing environmental standards.

Question 13. Concerning the maximum permissible average dose to tissue versus Dr. Radford's comments on a sharply localized source?

Answer to question 13. So far as an individual radioactive particle is concerned, the "average dose" to body tissue concept does indeed break down and therefore a new approach to standards in this case may be needed. With regard to genetic effects on DNA, the average dose to the body tissues may still be a useful concept if it can be shown that there is a one-to-one correspondence between the probability of irradiation of genetic material and the average dose in the body. In some cases, such as tritium or carbon-14, there is still disagreement on some aspects of this problem. Actually, the standards relate to the average permissible dose to a critical organ, in some cases the whole body and in others a specific tissue. The problem of combining risks has recently been discussed in ICRP publication 14. The sophisticated approach in this publication is in no way reflected in 10-CFR-20 MPC values or guidelines.

APPENDIX V

Contents:

MARCH 3, 1970.

Hon. GLENN T. SEABORG,
Chairman, Atomic Energy Commission,
Washington, D.C.

DEAR MR. SEABORG: As you are aware, the Subcommittee on Air and Water Pollution has been holding hearings on the underground uses of nuclear energy. These hearings have emphasized the potential environmental effects of radio- active release into the environment with particular emphasis on the use of nuclear explosives.

During these hearings there was discussion of a report prepared for the Atomic Energy Commission by the National Academy of Sciences—National Research Council. This report, dated May 1966, was purported to be a review of the waste disposal practices at five Atomic Energy Commission establishments (Savannah River Laboratory, Oak Ridge National Laboratory, Carey Salt Company mines, National Reactor Test Station, and Hanford Atomic Products operation). Be- cause this report would be a useful part of the Subcommittee record, both as it applies to this legislation and to other water pollution invesigations of the Subcommittee, I would appreciate receiving a copy at your earliest convenience.

Sincerely,

EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution.

U.S. ATOMIC ENERGY COMMISSION,
Washington, D.C., March 11, 1970.

Hon. EDMUND S. MUSKIE,
Chairman, Subcommittee on Air and Water Pollution, Committee on Public Works, U.S. Senate.

DEAR SENATOR MUSKIE: As requested by your letter of March 3, 1970, enclosed is a copy of the May 1966 report of the NAS-NRC Committee on Geologic Aspects of Radioactive Waste Disposal. Also enclosed is a statement prepared by the AEC staff reviewing some of the background relating to the May 1966 report.

In 1968 that Committee was replaced by the present NAS-NRC Committee on Radioactive Waste Management (CRWM). The CRWM recently visited the four AEC sites that are discussed in the May 1966 report (Savannah River Labora- tory, Oak Ridge National Laboratory, National Reactor Testing Station, and Hanford, Washington—the Carey Salt Company is not an AEC site although we have carried out some research an ddevelopment work there in leased facili- ties). At each site the CRWM received comprehensive briefings and tours of waste management activities and the Committee recently issued a report dated February 17, 1970 on these visits, a copy of which is enclosed. In that report it is stated that "The Committee noted the extensiveness and care in waste manage- ment at each site visited. The Committee is gratified by the quality and scope of the R&D program sponsored by the AEC in radioactive waste management."

There are additional and more current published descriptions of radioactive waste management activities at AEC sites than are contained in the May 1966 report which we will be happy to make available to your Subcommittee if you so desire.

Cordially,

GLENN T. SEABORG,
Chairman.

NATIONAL ACADEMY OF SCIENCES—NATIONAL RESEARCH COUNCIL REPORT TO THE DIVISION OF REACTOR DEVELOPMENT AND TECHNOLOGY, U.S. ATOMIC ENERGY COMMISSION

Committee on Geologic Aspects of Radioactive Waste Disposal: John E. Galley, Chairman; Charles W. Brown, George B. Maxey, John C. Maxwell, Charles Meyer, Robert C. Scott. Charles V. Theis, A. F. Van Everdingen; J. Hoover Mackin, Ex officio; Earl Cook, Executive Secretary.

I. INTRODUCTION

The Work of the Committee on Geologic Aspects of Radioactive Waste Disposal of the National Academy of Sciences-National Research Council (NAS-NRC) is supported by the Division of Reactor Develoment and Technology of the United States Atomic Energy Commission (AEC), whom the Committee serves as adviser. The Committee's responsibility to that Division is to observe and study critically the research and development activities of the Division with respect to radioactive waste disposals in the ground, and to provide counsel regarding the safety of the Division's current and proposed operations insofar as they are affected by geologic considerations.

The membership of the Committee has changed from time to time as earlier members were replaced by new ones. Of the present eight members, one has served on the Committee since its inception in 1955, and the three newest were appointed in March 1965. The professional backgrounds of the present members reflect a broad range of experience in the earth sciences. They are, respectively, hydrology, geohydrology (two members), radiohydrology, petroleum reservoir engineering, mining geology, structural geology and geophysics, and subsurface geology.*

Like all responsible citizens, the members of the Committee are concerned for the welfare of man and the perpetuation of an environment in which he can satisfy his physical needs and realize his cultural aspirations. The development of safeguards against hazards which might threaten the safety of man and his invironment is therefore a prime desire of the Committee. In pursuit of this broad objective of the Committee concerns itself with all phases of ground disposal of radioactive wastes, although its specific delegated responsibilities are the geologic aspects of the research and development program of the AEC's Division of Reactor Development and Technology. While this report, therefore, deals primarily with the several items of that program that are discussed in the chapter on "Research and Development Program of Division of Reactor Development and Technology," some observations on the general subject of radioactive waste disposal in the subsurface are outlined, with suggestions regarding research or other investigations which may be useful to personnel engaged in those activities.

II. HISTORICAL REVIEW

A. General

The Committee on Geologic Aspects of Radioactive Waste Disposal, first assembled in April 1955, originated as a steering committee whose principal function was to assist the AEC's Division of Reactor Development and its agents in a search for safe methods of ground disposal of hazardous wastes. From late 1955 until mid-1960 the Committee was known as the Committee on Waste Disposal; in January 1961 it acquired its present name.

In the beginning the Committee's specified responsibilities were limited to the disposal of high-level wastes, and in its conferences with other groups it was actively soliciting and evaluating ideas for any methods of disposal into the earth.

It early established for itself a set of guidelines which it continues to observe:

(1) Safety is a primary concern, taking precedence over cost.

*See Glossary for meanings of unfamiliar terms

(2) Radioactive waste, if disposed of underground, should be isolated as permanently as possible from contact with living organisms.

(3) The disposal of waste is a special problem for each particular installation.

(4) In addition to problems requiring urgent attention at existing plants, other problems will be created in the long-range development of power from atomic energy.

(5) Selections of future plant sites must include consideration of waste-disposal sites and methods, waste transportation and other potentially hazarduous operations. Study of a specific site requires complete geologic mapping as well as a combination of geophysical and geochemical studies.

B. 1955

In September 1955, the Steering Committee sponsored a conference at Princeton University (Ref: "The Disposal of Radioactive Waste on Land," NAS-NRC Publ. 519) at which 65 scientists representing many branches of earth sciences, biology and medicine, chemistry, physics, engineering, and other pertinent fields of knowledge, considered various problems of radioactive waste disposal on land and offered suggestions toward their solution. Among the proposed disposal methods which developed from this meeting were disposal in salt, deep-well disposal in permeable formations, and conversion of liquid wastes to solids. A conclusion of the Committee after reviewing the Proceedings of the Princeton Conference was that continuing disposal of low-level waste in the vadose water zone, above the water table, probably involves unacceptable long-term risks.

C. 1956

By the end of 1956, the Committee's activities expanded to include reviews of results obtained from research and development work carried out by the Atomic Energy Commission and its contractors, recommendations of necessary or desirable areas of investigation or specific projects, evaluations and recommendations regarding work proposal from responsible organizations, periodic reviews of operational experience, and assistance in the integration of available earth-science knowledge pertaining to waste-disposal problems.

D. 1958

One of the early results of the Committee's interest in subsurface disposal methods was the publication in July 1958 of a report, "Thermal Considerations in Deep Disposal of Radioactive Waste," by Francis Birch (NAS-NRC Publ. 588). Having considered the theoretical aspects of heat generation and dissipation from radioactive wastes that might be placed in deep permeable strata or in salt deposits, and after calculating the heat levels that would be reached, Birch concluded that—

"The results suggest that nearly any quantity of waste producing heat at the rate of 0.01 watts per gallon, if distributed in a layer of the order of 100 meters thick, can be accommodated without undue rise of temperatures. Concentrations much above this level would raise questions exceedingly difficult to answer. It does not appear that any decisive advantage is gained by burial at excessive depths (greater than a few thousand feet, for example) provided that potable water supplies and other natural resources are adequately protected."

In October 1958, when its area of responsibility had been broadened to include low-level and intermediate wastes, the Committee considered a proposal to inject radioactive wastes under pressure into artificial fractures in shale. At that time the Committee expressed doubt that this injection procedure would always produce horizontal fractures but agreed that the concept should be tested in the field. Meanwhile, Oak Ridge National Laboratory (ORNL), in Tennessee, had initiated plans to investigate possibilities for disposal in salt beds, and on request from AEC a subcommittee in the Division of Production, American Petroleum Institute (API), had recently completed a report on "Problems in the Disposal of Radioactive Waste in Deep Wells" (October 1958). The NAS-NRC Committee endorsed API's proposed program of studies for deep-well disposal, involving ion-sorption studies on naturally occurring sedimentary rocks, plugging, and corrosion and radiation damage, as well as regional geologic studies to highlight favorable areas for more detailed analysis. At the same time it expressed continuing concern about the disposal of intermediate and low-level waste liquids in seepage pits at ORNL because they contaminate the environment. It concluded that low-level waste is particularly amenable to disposal in deep porous media or possibly in moderately permeable shale, and it urged a long-

range effort to find sites and methods of treatment so that these wastes can be emplaced safely at depth.

E. 1960

A proposal to construct bedrock storage facilities for high-level liquid wastes at the Savannah River Plant (SRP), South Carolina, was brought to the attention of the Committee in March 1960, it having first been submitted to AEC in 1958. Recognizing that the wastes must be contained indefinitely and that grouting, sealing or lining might be necessary, the Committee recommended that SRP proceed with test borings, and that the project then be reconsidered after the results of the tests were available. The Committee noted that the investigation should include study of the sedimentary section and hydrologic conditions above the basement, and that within the basement the studies should include the extent of fractures, water content and conditions, and the physical and chemical properties of the rocks.

The Committee commended the program of studies in hydraulic fracturing at ORNL and the progress on investigations toward deep disposal in salt, which project had progressed from laboratory to field tests in a salt mine in Kansas.

During the same meeting the Committee expressed concern over the fact that no existing AEC installation is in a geologically acceptable location for disposal of highly radioactive liquid waste and that future waste-producing plants might also be placed in unfavorable locations if the feasibility of safe ultimate disposal is disregarded. The Committee also expressed its belief that there is serious need to get beyond the problems of disposal at existing installations that were located with little regard to ultimate disposal procedures, and to give additional emphasis to the job of finding safe permanent disposal sites.

In May 1960, the Committee visited the Carey salt mine near Hutchinson, Kansas, where ORNL was conducting its investigations on the feasibility of disposal in salt. Studies at that time were still being directed toward disposal of high-level liquids in salt, although attention was beginning to turn to development of a process for calcination of high-level liquid wastes in stainless-steel pots suitable for subsequent burial.

In view of increasingly unsatisfactory results of disposals into seepage pits at ORNL, the Committee reiterated its concern about that type of disposal, observing that similar disposal methods were being used at the National Reactor Testing Station (NRTS), Idaho, and at Hanford Atomic Products Operation (Hanford), Washington.

Still concerned about disposals into seepage pits, the Committee visited the Hanford and NRTS sites in June and July 1960. Its conclusion as a result of the visits was that neither location had been shown to provide safe and permanent disposal, although some confidence was expressed by the local staffs, based on the fact that the sites are located in areas of little rainfall. It commented that

"The protection afforded by aridity can lead to over-confidence: at both sites it seemed to be assumed that no water from surface precipitation percolates downward to the water table, whereas there appears to be as yet no conclusive evidence that this is the case, especially during periods of low evapotranspiration and heavier-than-average precipitation, as when winter snows are melted. At the National Reactor Testing Station pipes were laid underground without ordinary safeguards against corrosion on the assumption that the pipes would not corrode in the dry soil, but they did. At NRTS plutonium wastes (plutonium half-life 24,000 years) are given shallow burial in ordinary steel (not stainless) drums on the same assumption. Corrosion of the drums and ultimate leakage is inevitable. * * *

"The movement of fluids through the vadose zone and the consequent movement of the radioisotopes are not sufficiently understood to insure safety. The work in progress is commendable and deserves encouragement. The mounting pace of nuclear developments will add to the pressures on waste disposal facilities, procedures, and research at Hanford and Idaho Falls, and the future emphasis should be on safe ultimate disposal viewed from the long range and with attention to a rapidly changing world. * * *"

F. 1961

The Savannah River Plant was visited by the Committee in December 1961, when four test holes had been drilled into the crystalline "bedrock" and the logging and testing of the first had been completed. At this early stage of the investigations the nature and extent of fractures in the bedrock were not clear, and the hydrologic system had not yet been determined. In the general discussion,

doubt was expressed that the ideal, absolutely dry, opening in the basement rock could be attained, and the problem of protecting fresh water from contamination loomed large. The Committee recommended that much more precise information on the amount, rate, and direction of movement of water in the area under consideration be obtained; types of recommended measurements and tests were specified; steps for obtaining more information on the orientation of structures in the crystalline bedrock were urged.

Reports also were heard on the progress of fracturing experiments at ORNL and on studies of various types of grout mixes. The Committee commended the present status of the project, but stressed the importance of learning the orientation of the hydraulically induced fractures. It further cautioned that the specific information produced by hydraulic-fracturing experiments in folded and faulted rocks of a heterogeneous sequence such as that at Oak Ridge cannot be extrapolated safely from one formation to another or from one area to another.

Further laboratory and field studies on the disposal of radioactive liquids in salt were reported, and the Committee learned that the project, which had been initiated soon after the Princeton Conference, was now being returned to the original objective of determining the phenomena that might be associated with the storage or disposal of solid wastes. The Committee was satisfied with the progress of the studies, and recommended that the effect of storing dry, packaged, radioactive wastes in a salt bed be tested as soon as possible.

Proceeding further, the Committee noted:

"The containment of fission products in an inert solid is still the manner of waste disposal that the Committee most favors. Research and development designed to produce a material from which fission products cannot be leached should be encouraged and supported as long as there is a reasonable possibility of attaining the desired objective. Nevertheless, the Committee still supports the idea that liquid wastes may be disposed of with safety into deep synclinal basins, below the level of the lowest potable water."

G. 1963

At a meeting in Washington in June 1963, the Committee heard reports regarding the Savannah River bedrock-storage proposal, the grout-injection experiments at ORNL, and the feasibility studies of disposal in salt; much time was devoted also to reports of studies of deep-well disposals in permeable formations. These are outlined in more detail in the following paragraphs.

SRP personnel provided the Committee with volumes of data that had been derived from 2½ years of careful drilling and testing of exploratory boreholes. The ultimate objective of the research was to determine the hydrologic characteristics of subsurface sedimentary and crystalline rocks at the plant site and the various other parameters that, it was hoped, eventually would determine whether or not the crystalline bedrock would be a suitable reservoir for the storage of high-level waste liquids. The Committee commended the SRP staff on the progress that had been made in its investigations, and expressed its opinion that for long-term safety, underground disposal at this locality is much better than storage in near-surface tanks. The Committee also observed that much work remained to be done before the safety of a bedrock storage chamber could be demonstrated, and it offered suggestions regarding further steps that would be desirable. It recommended that work be started on the next phase of the program, and that SRP personnel visit existing mines in crystalline rocks of the Atlantic Coast piedmont in order to study the bedrock as exposed in mine openings and to observe some of the problems of water control in such chambers.

Progress in the development of technology for artificial fracturing and grout injections at ORNL appeared to be satisfactory. A number of experimental injections had been made, using simulated wastes, and the location, shape and extent of the resultant grout sheets had been determined. No evidence of vertical fractures had been found, and it was reasonably certain that the ideal of producing horizontal fractures had actually been attained. The Committee recommended initiation of a pilot-plant operation for disposal of actual low-level wastes as the next stage in the continuing experiments. Further, because of the danger of creating vertical fractures by high-pressure injections at depth, the Committee recommended that disposals be limited to the relatively shallow red-shale member of the Conasauga Formation.

Investigations toward disposal of high-level liquids in salt beds had been discontinued since 1961, and attention in the Kansas salt mines had been directed instead to the problems of storing high-level solids in salt. Experiments designed to determine the heat-diffusion characteristics of the salt beds and the effects

of radiation on the salt were under way. Continuation of the studies toward ultimate disposal of high-level solid waste in salt was recommended by the Committee.

In connection with AEC's research program regarding disposal of liquid wastes by injection into deep permeable formations, the Committee heard reports on a series of studies of geologic basins initiated to select a site for pilot-plant injection operations in a deep disposal reservoir. Individual basin reports were being prepared by personnel of the U.S. Geological Survey (USGS), together with a summary report on all basins in the conterminous United States. Simultaneously a study of six basin provinces was being conducted by a subcommittee of the American Association of Petroleum Geologists (AAPG). The six provinces had been selected on the basis of geologic conditions in each province that might be suitable for three different types of underground disposal: (1) Shallow salt beds for high-level solids, (2) shale strata at depths suitable for injection of grouted intermediate-level wastes into hydraulically produced fractures, and (3) a deep permeable sandstone for injection of low-level waste liquids.*

Dr. W. J. Kaufmann described experimental work being conducted at the University of California where a group of injection and relief wells in a five-spot pattern within a 36-foot square plot of ground were drilled to depths of about 95 feet in a thin (3 to 7 feet) confined layer of unconsolidated sand and gravel. Studies had been made of the movement of tritiated water, of water containing Strontium 89, and of the radioisotope solutes themselves as their progress was retarded by sorption on the aquifer surfaces.

In order to extend the studies into deeper subsurface systems which might more closely resemble the deep permeable reservoirs toward which AEC had been looking for possible disposals, a field study had been proposed in which fluids would be injected into a permeable sandstone at a depth of a few hundred feet. A site for this purpose had been selected near Bartlesville, Oklahoma, where the U.S. Bureau of Mines would conduct injection tests in the Cottage Grove sandstone member of the Pennsylvania Chanute Formation. The description of the stratigraphic section, as presented to the Committee, indicated that the objective sandstone is underlain by a coal bed. Inasmuch as the permeability of coal is likely to be fairly high, the Committee suspected that the coal would probably act as a bypass for injected fluids, in which event the experiment would fail. A somewhat deeper sandstone, the Wayside Member, was considered too thick for satisfactory experimentation, and because it is overlain by a limestone stratum of doubtful quality as a seal it was considered even more unsuitable. The Committee judged that although the proposed injection experiment *per se* probably would provide some of the desired results, the chosen site probably was unsuitable, and recommended that a more favorable geologic location be sought if subsequent testing confirmed this evaluation.

H. 1965

The present report of the Committee stems from a series of meetings that began on April 12 to 15, 1965, when the Committee visited plant sites at the Savannah River Laboratory, South Carolina, and at Oak Ridge, Tennessee. At Savannah River the Committee was briefed thoroughly on the present status of the investigation into prospects for storage of high-level liquid wastes in the Precambrian crystalline bedrock; it visited current disposal operations including the open seepage basins for low-level liquids, trash burial grounds, and the storage tanks for high-level liquids. All of the operations were discussed in detail during indoor sessions. Before leaving Savannah River the Committee also heard briefly from Dr. E. H. Baltz regarding present efforts by USGS to find a site suitable for subsurface injection experiments of the type attempted at Bartlesville, Oklahoma.

At Oak Ridge the Committee visited the site of grout-injection operations and learned of developments since its 1963 meeting. Downhole logging after one injection had revealed gamma activity above the grout sheet, which may have been the result of migration of the grout upward between the well casing and wall of the hole or may have been evidence of vertical fracturing; however, because formation water had flowed back into the hole after the injection, the gamma activity was more probably the result of contamination from that

*The results of this study have been reported in "Radioactive waste-disposal potentials in selected geologic basins," USAEC Div. of Technical Information Report SAN-413-2 (1964).

source. Additional briefings and discussions included studies of fixation of radionuclides on solids and in emulsified asphalt, the movement of nuclides in groundwater streams, the programs for converting high-level liquids to solids, and various current waste-management practices such as the use of seepage pits and trenches, the use of burial grounds for radioactive trash, the construction of an evaporator plant, and the monitoring of radioactivity both at ORNL and in the surrounding countryside.

On May 10 the Committee visited the Carey Salt Company mines at Hutchinson and Lyons, Kansas, to observe experimental work on storage of high-level radioactive solids in salt beds. It then travelled to Idaho Falls and Arco, Idaho, for a 1½-day review of research programs and operations at NRTS. Because of the large number of facilities and the diversity of operations at NRTS, the range of subjects discussed and processes observed was broad. The subjects includes the ground-water hydrology of the Snake River Plain and the NRTS reservation, to which much time was devoted during the visit; the need for a deep (5000-foot) exploratory bore-hole; tritium-tracer studies of the movement of ground water and injected wastes; ion-exchange phenomena; present disposal practices for both liquid and solid wastes of high, intermediate and low levels of radioactivity; monitoring procedures; calcination processes for conversion of high-level liquids to solids; and considerations of costs and economy.

Although impressed by the competence and dedication of the NRTS staff in its efforts to solve many vexing disposal problems, at the conclusion of its visit the Committee departed with two unrelieved major anxieties. (1) that considerations of long-range safety are in some instances subordinated to regard for economy of operation, and (2) that some disposal practices are conditioned on over confidence in the capacity to the local environment to contain vast quantities of radionuclides for indefinite periods without danger to the biosphere. These concerns will be discussed further in this report.

On May 13 and 14 the Committee examined disposal operations and installations at the Hanford Atomic Products Operation, Washington, and discussed research programs and operational practices with local staff. As at NRTS the spectrum of waste-disposal projects at Hanford is a wide one. Subjects that were discussed included the geology and ground-water hydrology of the Hanford site and the Pasco Basin; research in hydrology with respect to ground disposals at Hanford, both for saturated and for unsaturated flow; studies pertaining to anticipated changes in the hydrology after completion of the Ben Franklin Dam on the Columbia River imposes its influence on the ground-water levels; soil chemistry and minerology of the Hanford site; the migration of radionuclides in the subsurface, their monitoring and control; current waste-disposal practices for radioactive trash and low, intermediate and high-level liquids; progress in development of technology for calcination of high-level liquids and disposal of the solid products. Tours of the Hanford site enabled the Committee to observe various installations and procedures.

As at NRTS, the Committee was concerned over the prevailing belief that the unconsolidated soil, sand and gravel that comprise the surficial materials to a depth of several hundred feet provide a reservoir for safe storage of tremendous quantities of wastes of all levels of radioactivity, and that no hazardous amounts of radioactivity will percolate down to the water table. The inadequacy of present understanding of hydrology in unsaturated earth materials (the vadose zone) was generally recognized.

The Committee assembled in Washington, D.C., on June 7 to 9, primarily for the purpose of drafting its report to AEC, but it also took advantage of an opportunity to receive further briefing on developments in AEC's deep-well disposal program, particularly the completion of the basin studies by USGS and AAPG. It also heard and considered a proposal for preparation of a subsurface reservoir that would be held in standby readiness in the event of an unanticipated release of radioactive gas from a plant accident.

Inasmuch as certain aspects of the report draft required further investigation and prolonged study by the Committee, another meeting was held on August 30 and 31, in Dallas. This was solely a discussion and work session, and the final drafting of this report has been done subsequently. The Committee's appraisals, comments and recommendations pertaining to the research and development programs and the waste-disposal practices that were reviewed in all of the 1965 visits and meetings are contained in the following pages.

I. Conclusions

Throughout the fabric of the 10-year history of the Committee's deliberations run some continuing threads of purpose and conviction. Prominent among them is the realization that none of the major sites at which radioactive wastes are being stored or disposed of is geologically suited for safe disposal of any manner of radioactive wastes other than very dilute, very low-level liquids, with the probable exception of grout injection into fractured shale at Oak Ridge. Another is the knowledge that the safety and the needs of populations in centuries to come demand that methods and facilities be developed now to receive and contain safely in perpetuity the volumes of wastes that will be produced as the nuclear industry expands the sites for new plants are selected and developed.

Research and subsequent development of promising techniques have provided a series of improvements in disposal methods that augur well for the handling of the larger amounts of radioactivity that are expected to be produced beyond the year 2000. These are bright strands in the warp through which are woven the observations and recommendations of the present report.

III. ACKNOWLEDGMENTS

As related in preceding paragraphs, during April and May 1965 the Committee was privileged to visit operations at five sites where radioactive-waste management practices are being developed, these being the Savannah River Plant (SRP), Oak Ridge National Laboratory (ORNL), the Lyons salt mine of Carey Salt Company, the National Reactor Testing Station (NRTS), and Hanford Atomic Products Operation (Hanford or HAPO). The Committee appreciates the hospitality and cooperation that were shown by the staffs at all sites. They generously supplied copies of reports and data, and patiently answered a multitude of questions. Additional useful information has been provided by staff members of AEC and USGS.

IV. OBSERVATIONS

A. General

The Committee is favorably impressed by the competence, dedication and sincerity of all of the people in AEC and its contractor companies who are responsible for the safe handling of radioactive-waste materials. We are happy to acknowledge the conscientious efforts that generally are being applied to the problems of safe storage and disposition of hazardous materials. We particularly commend the progress on the solidification of the high-level and intermediate waste liquids, the extension of the program of disposal in salt to a field study, and the continuing developments in technology for disposal by grout injections.

In radioactive-waste disposals all plants operating under AEC surveillance are guided by the radiation-protection standards that have been developed by the Federal Radiation Council (FRC). These standards are defined specifically in reference to such terms as the Radiation Protection Guide (RPG), Radioactivity Concentration Guide (RCG), and Portective Action Guide (PAG). In operations that have been observed by or described to the Committee, efforts are directed toward restricting radioactivity to a fraction of the amounts permitted by the FRC standards.

The choice of treatment and disposal procedures for radioactive wastes is based not only on the characteristics of the radionuclides and the chemical and physical properties of the media in which they are contained, but also on the total amount of radioactivity in the wastes. A scale of radioactivity commonly used in AEC's Division of Reactor Development and Technology is the following:

Low-level: Less than one microcurie per gallon.
Intermediate: One microcurie to one curie per gallon.
High-level: More than one curie per gallon.

On the other hand, at each site the staff has established its own scale of levels of radioactivity in waste materials, and these are not entirely uniform from site to site. For example, intermediate-level wastes in some places may contain as much as 10 curies per gallon. Straub ("Low-level radioactive wastes," AEC Division of Technical Information Publication, 1964) cites a usage in which low-level wastes have activity measurable in micro-curies per liter or per gallon, intermediate levels measurable in millicuries per liter or per gallon, and high levels in curies per liter or per gallon.

Another suggested "classification," adapted from other industrial operations, is based directly on methods of handling and disposal, rather than on a numerical scale of concentrations:

Concentrate and contain (high-level wastes).
Delay and decay.
Dilute and disperse (low-level wastes).

To the extent that consideration is given to the longevities of the various radioisotopes in the wastes, such a classification appears to be applicable to radioactive-waste disposal. It is, however, not a classification of wastes but rather a summarization of concepts for methods of handling them. All three concepts require isolation from the biosphere of radionuclides in hazardous concentrations. To "concentrate and contain" carries the stringent requirement that the container must be leak-proof throughout the period of hazard. Because artificial containers are subject to corrosion, fracture, and other forms of damage over periods of centuries, safe natural containers totally and permanently separated from the zone of fresh water must be used. To "delay and decay" requires that isolation from biosphere be complete during the period when hazardous concentrations exist. To "dilute and disperse" requires particular attention to the quantities of long-lived radio-nuclides in the wastes, especially in view of prospects that concentrations in the ground may increase cumulatively through the years, and that changes in soil chemistry or hydrologic patterns may release the sorbed nuclides. To the extent that radioactive decay exceeds input, there is no reason for alarm.

The Committee acknowledges AEC's awareness of the potential hazards of cumulative build-up of radioactivity from long-lived nuclides, as well as careful monitoring that is part of the routine at disposal sites to avoid such danger. The procedure includes recording of the amounts of all radionuclides in the waste streams, sampling and analysis of fluids obtained from monitor wells in disposal areas, and computation of activity held in the ground. The continuation and improvement of such procedures at all sites, including privately owned plants to be built in both the immediate and the far future, are anticipated.

In view of the hazards inherent in a cumulative build-up of *long-lived* radioactivity above or in a fresh-water aquifer after decades of a disposal operation, it might be well to consider the use of a scale of radioactivity based on the longevity of nuclides in the waste as well as on the concentration of activity.

B. GEOLOGY

1. Savannah River Plant

The Savannah River Plant occupies about 315 square miles in South Carolina, 20 miles southeast of Augusta, Georgia. The site lies in the Atlantic Coastal Plain physiographic province, near its inner edge where the wedge of coastal plain sedimentary strata is relatively thin. Intake areas for the fresh-water aquifers in the strata lie a short distance westward, the edge of the Piedmont province being only a few miles upstream from Augusta. The average annual precipitation is 43 inches. The topography is one of rolling hills with local relief as much as 250 feet; maximum surface elevations are about 350 feet above sea level.

The stratigraphic section has a total thickness of about 1000 feet above the Precambrian basement and consists of unconsolidated and partly consolidated sedimentary strata ranging in age from Upper Cretaceous to Recent. The thickest unit is the Tuscaloosa Sandstone (Upper Cretaceous), near the base of the sedimentary section. It consists of about 600 feet of gravel, sand and intercalated clay beds and contains the largest volume of fresh-water reserves in the area.

Between the Tuscaloosa Sandstone and the Precambrian rocks is a thin layer (average about 70 feet) of saprolite clay which presumably is a residual from decomposition of the Precambrian crystalline rocks and spread across the surface of the costal plain as encroaching seas advanced across the area in late Cretaceous time. Although the thickness of the saprolite layer appears to be fairly uniform, data from boreholes reveal places where the layer seems unusually thin. The log of borehole (DRB-2) in the SRP test series failed to show any of the saprolite clay, but accuracy of the driller's log has been questioned. There may be buried channels or broader openings in the saprolite layer which interrupt its continuity, but none have been revealed clearly by the sparse data from scattered boreholes. In view of the small area that would be occupied by a channel, and in the present stage of subsurface technology, recognition of the presence of old channels could come only through fortuitous circumstances

in which more than one boring would by chance penetrate an identifiable "window" or hole in the clay layer.

Underlying the saprolite clay is the basement of Precambrian rocks which consists chiefly of foliated gneiss and schist with lesser amounts of quartzite and phyllite. The rock is fractured in varying degree, the upper sections being more highly fractured than deeper ones.

2. Oak Ridge National Laboratory

This plant site of 92 square miles, about 25 miles west of Knoxville, Tennessee, lies in the Valley-an-Ridge physiographic province between the Great Smoky Mountains on the east and the Cumberland Plateau on the west. The topography within the site area is characterized by parallel asymmetric mountain ridges separated by valleys whose floors are rolling or hilly. Topographic relief between ridges and valleys is generally in the range of 200 to 350 feet: surface elevations are between 800 and 1150 feet above sea level. The average annual precipitation is about 47 inches.

Except for thin covers of Recent alluvium the surface is cut mainly in Cambrian and Ordovician strata, which form the bedrock both on the ridges and in the valleys. As elsewhere in the Valley-and-Ridge province the Paleozoic strata have been subjected to lateral compression to the extent that much severe folding and thrust faulting have occurred; the topography is mostly a reflection of the tectonic structures. Abundant jointing and other fracturing of competent strata, as well as bedding-plane slippages, are to be expected. Fresh-water supplies are found at relatively shallow depths in Paleozoic strata and locally in the Recent alluvium. Deeper waters are brackish or saline.

3. National Reactor Testing Station

The area enclosed within the boundaries of NRTS amounts to about 894 square miles lying in southeastern Idaho, some 30 to 60 miles west of Idaho Falls. It is semi-arid land, the average annual precipitation being between 7 and 10 inches. The physiographic province is the Snake River Plain, a subdivision of the Columbia Plateau, about 200 miles long from northeast to southwest and 50 to 60 miles wide. The east and south margins of the Plain are drained by the Snake River: elsewhere the drainage is largely centripetal, the surface water disappearing by a combination of evaporation and seepage into the ground in areas of topographic depression. Streams such as the Big Lost River entering the Plain from mountains on the west and north debouch into mud flats or prairie sinks.

The surface of the Plain, which at NRTS is about 4800 to 5000 feet above sea level, is generally flat or hummocky except that here and there are a few buttes rising several hundred feet, in one instance more than 2000 feet, above the Plain. The surface rock over large areas is black basalt of Recent origin; some flows are still fresh, unaltered by weathering and bare of vegetation. Elsewhere the lava is covered by a thin veneer of soil derived from decomposed lava, gravel, alluvial or aeolian sand and silt, or in some places lake beds or playa deposits. Beneath the dark basalt are flows of older, paler, acid lavas.

The subsurface sequence of layered rocks, to an unknown depth, consists of lavas interbedded with pyroclastic and clastic sedimentary strata. The basement under them presumably consists of indurated Paleozoic rocks similar to those that can be seen in outcrops in adjacent mountain areas. They probably are strongly folded and fractured. Some of the lava beds above the basement are remarkably porous and permeable; together with beds of sand and gravel they are the chief aquifers of the province. It is estimated from discharged data that more than 13,000 acre-feet per day of water flows through these aquifers under the Snake River Plain. The direction of flow is southwestward, and a large portion of the water appears again at springs along the Snake River at the southwest end of the Plain. A conspicuous local feature there, near Hagerman, is known as the Thousand Springs, where an estimated volume of 500 cubic feet of water per second gushes from a basalt layer that crops out in the escarpment 185 feet above the river level.

4. Hanford atomic products operation

The Hanford site occupies 567 square miles in southeastern Washington about 30 to 50 miles east of Yakima on the Columbia Plateau. It lies in the Pasco Basin subdivision, a topographic depression between the Saddle Mountains on the north, Rattlesnake Hills and Horse Heaven Hills on the west and southwest, and

the Blue Mountains of Oregon on the southeast. Within the site boundaries, surface elevations above sea level range from about 350 feet at the Columbia River to 600 feet at the base of the Rattlesnake Hills. Topographic relief within the basin is low, the surface being generally hummocky except for the ridge of Gable Mountain, in the north part of the site, which rises some 600 feet above the plain. Rattlesnake Hills, at the southwest edge of the site, have a maximum elevation of 3500 to 3600 feet above sea level.

In the broader view, the site is in a structural basin of southern Washington and northern Oregon which was filled in late Tertiary (Miocene and Pliocene) and Quaternary times with thousands of feet of lava, pyroclastics, and interbeds of continental gravel, sand and silt. The base of the layered series lies at an unknown depth more than 10,000 feet below the surface. The nature of the rocks beneath the volcanic series is unknown; presumably the basement rocks are highly indurated and deformed Mesozoic and Paleozoic rocks similar to those seen in outcrops at the margins of the Columbia Plateau. The interval between the basement and the base of the lava series is a matter of speculation as to thickness, lithology, age, and fluid content.

All permeable strata that have been tested, lava as well as sedimentary beds, are fresh-water aquifiers. In spite of the semi-arid climate (between 6 and 9 inches average annual precipitation), the reserves of fresh water in this great thickness of acquifiers are enormous, most of the supply being derived from adjacent mountain heights.

The lava series is overlain in large areas by beds of gravel, sand, silt, and volcanic ash which constitute the upper part of the Ellensburg Formation and the Ringold Formation, and these in turn by late Quaternary alluvium and lake beds. These beds above the lava series are acquifiers, in which porosity and permeability are adequate. They and the underlying, permeable lava flows and interbeds are being used as reservoirs for ground disposal of radioactive wastes.

5. Comparative geology

It has been pointed out by R. M. Richardson * that conditions at SRP and ORNL are alike in respect to the fact that the water table at each site is shallow (a few feet or tens of feet) and the distance from any disposal operation to a point of ground-water discharge is short (hundreds of yards) further, that NRTS and Hanford are alike in that the water tables there are deep (hundreds of feet) and the distances to points of discharge are relatively great (miles). The different conditions result from the difference between humid and semi-arid climatic regimes. The operations at Lyons, Kansas, are exempt from such considerations being restricted to salt beds far beneath fresh-water aquifiers.

Another set of tentative similarities, subject to confirmation by deep exploratory drilling at NRTS and Hanford, also should be recognized. Geologic conditions at SRP, NRTS, and Hanford probably are alike in respect to the fact that the fresh-water aquifiers at each site occupy a thick sequence of relatively undisturbed layered rock lying directly on a basement of less permeable, folded, faulted and fractured "bedrock." At Savannah River the water in the basement contains a total dissolved solids concentration of about 6000 p.p.m., in contrast to about 34 p.p.m. for water in the overlying layered aquifers. In other respects also the two rock systems appear to contain different hydrologic systems. The layered rocks at Savannah River, about 1000 feet thick, all lie in the zone of saturation.

At NRTS and Hanford no borings have penetrated the complete thickness of layered rock, and the degree of folding and fracturing in the underlying basement is unknown. Likewise the presence of any salaquifers above the basement is now known. The deepest borings in the Snake River Plain (NRTS) is only 1500 feet deep, and the deepest in the Pasco Basin (Hanford) is 10,655 feet. Only fresh water has been found in analyzed samples from either province, and the depth to which fresh water extends therefore has not been ascertained at either place. Between the water table and the thin root zone is a zone of unsaturated basalt and unconsolidated sediments, the so-called "dry soil," or zone of aeration several hundred feet thick. Nothing is known about the water regimen in the basement rock, or about any possible flow barrier between the basement and the overlying layers.

*Significance of climate in relation to the disposal of radioactive waste at shallow depth below the ground," Proceedings of International Conference on Retention and Migration of Radioactive Ions Through the Soil; Saclay, France, Oct. 15–19, 1962.

At ORNL, on the other hand, the folded, faulted and fractured bedrock composed of distorted sedimentary layers, is near or at the surface and contains fresh-water aquifers. Only a veneer of loose sediments, also containing fresh water, lies above it.

In contrast to these four sites, there are large areas in the United States, particularly in the stable interior of the continent, in which fresh-water aquifers are separated from the basement by many thousands of feet of layered strata which contain saline waters and are not likely to be drawn upon for water supplies. It is in such areas that disposal of radioactive liquids into permeable subsurface strata can more firmly be relied on as a safe procedure.

The above similarities and distinctions point out the major characteristics of the geologic environments on which decisions regarding disposals at the present plans must be based.

C. *Hydrology*

At all sites the Committee noted insufficient knowledge of or, at least, too little consideration of the three-dimensional movement of water and contained materials in porous media. Usually it is the vertical dimension that is poorly understood, and in the establishment of flow paths and velocity of movement this dimension is highly important.

V. GENERAL PRINCIPLES

The deliberations of this Committee continue to be guided by the basic rule that concentrations of radionuclides in waste materials should not be allowed to appear in the earth's biosphere before they have decayed to innocuous levels. This concept requires assurance that during any storage or disposal* operations hazardous amounts of nuclides are isolated from the biologic environment, and that upon completion of the procedures the nuclides will remain isolated as long as they might constitute a hazard. For some nuclides this requirement means isolation for periods of 600 to 1000 years, periods so long that neither perpetual care nor permanence of records can be relied upon. *All supplies of potable*** *ground water, whether or not they are now being drawn upon, are considered as being part of the biosphere.*

It is recognized that radioactivity in concentrations less than RCG is allowable, and within these limits the Committee has no concern. It is the possibility of cumulative build-ups of long-lived activity that may exceed these limits after continued use of doubtful practices, and the prospect of unforeseen concentrations in excessive amounts resulting from unexpected and uncontrollable alterations in the environments in future decades that the Committee wishes to guard against. In this report, terms such as "excessive amounts," "excessive concentrations," and "hazardous accumulations" are used with reference to excesses beyond the limits established by the Federal Radiation Council.

The economics of disposal and storage operations are of concern, but they are relegated to second-rank consideration, safety being the matter of first concern always.

All studies of waste disposal below ground level have borrowed heavily from the geohydrologic knowledge developed for the production of water from aquifers and the production of oil and gas from petroleum reservoirs. However, whereas the hydrodynamics of fluid production largely involve a concept of statistical homogeneity of the formations involved, the disposal of an insidiously dangerous waste involves the necessity of tracing or predicting its course through all the intricacies of the nonhomogeneities of these beds. Hydrodynamic concepts including dispersion, sorption, and erratic flow in all types of sediments and rocks in nature need development and documentation. Such problems are properly first studied in the laboratory, but some of them cannot be brought into the laboratory and all of them need checking in the field.

In view of the clear prospect for ever-growing production of atomic energy and therefore the probability of increasing output of radioactive wastes, the

*As far as possible in this report the word "storage" means emplacement with the intent and in such manner that the materials can later be retrieved. "Disposal" means emplacement in a manner or location that renders them practically irretrievable. In practice the two words often are used interchangeably, especially where the intent is not clear or retrievability is uncertain.

**Here "potable" refers to the quality of water which is now being consumed or eventually may be consumed by humans.

Committee believes that safe disposal procedures should be established now rather than at later stages of power development. Because perpetual care of hazardous wastes cannot be assured, storage and disposal procedures should be so designed that permanent safety is certain. At the same time, land areas, water supplies, and other resources that are appropriate for permanent storage or disposal of hazardous wastes should be the least quantities that are compatible with the prime objective of perpetual safety.

In the last analysis, it is the property of mobility of hazardous radio-nuclides in any fluid medium that baffles all who are concerned with safety of disposals. Operators will, therefore, continue to face dilemmas in waste disposal until hazardous radioactive waste is reduced to solid form in the smallest possible volumes. This is the concept the Committee favors above all others: a procedure of disposal involving concentration of radioactive solutions, their conversion to almost insoluble solids, and subsequent burial far below fresh-water aquifers in almost impermeable rock such as salt.

The present larger installation of AEC represent widely varying types of waste production and disposal. Anything learned at these sites will be of increasing importance in the future when waste-disposal problems will be magnified. AEC quite properly has considered that it has the responsibility of developing all types of reactors and of assessing the safety and economics of each type. We believe that it has also the responsibility of studying in detail all of the phenomena involved in the disposition of wastes to the environment, not only on the basis of assessing any known or suspected hazard, but more broadly on the basis of founding a discipline of waste disposal that will be of increasing importance in the future, especially when private operators may dispose of such waste and consequently the profit motive will become more prominent. We believe that there should be no phenomenon involved in any of the waste-disposal schemes that is not completely understood.

VI. RESEARCH AND DEVELOPMENT PROGRAM OF DIVISION OF REACTOR DEVELOPMENT AND TECHNOLOGY

A. *Disposal in salt*

1. *Introduction.*—Storage or disposal of radioactive wastes in underground cavities in salt deposits was discussed at the Princeton Conference in 1955, and in succeeding years this concept has been developed to a stage approaching operational use.

Definite advantages in this method of disposal are inherent in the unique physical properties of halite, the chief mineral in most natural salt deposits. The principal advantages are that salt is impermeable, soft, plastic, and easy to mine; in addition, it is an efficient natural shield against gamma radiation and is not associated in the earth with potable water.

2. *Early research.*—During early stages the investigation involved disposal of high-level *liquid* wastes in salt caverns. Studies began with laboratory tests of the physical properties of halite and the effects of heat and atomic radiation on those properties. At the same time, theoretical considerations of heat generation in liquid wastes and of thermal properties of subsurface salt deposits in which the wastes might be stored, were examined by Francis Birch. Preliminary conclusions from these studies were that heat-transfer characteristics, shielding properties, and other parameters are suitable for the use of salt as a disposal reservoir, and that salt flow or creep as a result of heating or radiation would not be so rapid as to endanger storage operations in cavern. Experiments on the radiolytic production of chlorine from salt indicated that no serious problems from this source in a mine would be expected.

Field experiments then were conducted in a disused mine of the Carey Salt Company near Hutchinson, Kansas, in which simulated acidic and neutralized Purex wastes were placed in salt chambers for periods of several weeks to months. Some cavity alternations resulted, but a more serious effect was the radiolytic production of hydrogen and oxygen. While studies of radiolytic gas production were continuing, the advent of techniques for solidification of high-level liquids led to diversion of the major effort toward experiments in the salt storage of these more manageable, solid, wastes. The first of these experiments were concluded in existing chambers, where tests were performed on the rate of creepage and the effects of heat on those rates.

The salt beds in central Kansas are of Permian age, part of the Wellington Formation in the Leonard Series. The salt is closely associated with anhydrite, mag-

nesite, dolomite, and shale. It consists chiefly of halite, but polyhalite occurs in small amounts, and other accessory minerals are minor impurities. Widely separated thin layers of clay form dark bands up to one inch thickness in the otherwise pale salt rock.

Field research more recently has been transferred to a mine of the Carey Salt company near Lyons, Kansas. The total thickness of the salt section here is about 300 feet, and the chambers excavated in the Lyons mine are about 1000 feet below the surface.

3. *Current research*.—Preparations are being made in the Lyons mine, in a new chamber constructed for this purpose, to test the effects of heat and radiation in simulated operational procedures for the storage of solid wastes by emplacement of irradiated fuel elements obtained from the Engineering Test Reactor at Idaho Falls. A pattern of seven holes will be drilled in the floor of the chamber, one in the center of the pattern and six in a circle around it, spaced five feet apart. The holes are to be one foot in diameter, 13 feet deep, lined and shielded, and designed each to contain two fuel elements in canisters. The assemblies will be replaced by fresh ones at six-month intervals over a period of two years. Supplementary heat will be provided artificially in order to maintain a temperature of 200°C as the heat from the fuel elements declines. This temperature is the selected maximum temperature desired, because naturally trapped moisture in the salt produces a shattering of the salt when temperatures approach 250°C.

In another part of the chamber a similar array of electrical heaters will be installed for the purpose of distinguishing those effects due ot heat alone.

Further studies of creep or flow are being conducted by measuring changes in the shape and dimensions of mine pillars, walls, ceiling and floor. The measurements are obtained by the use of both internal and external gauges. Preliminary results indicate increases in flow rates with heating that conform closely to laboratory results. The initial rates of closure of mined openings are high but the decrease with time. Heating increases the rates markedly, the effect of elevating the temperature being essentially the same as that of increasing the pillar load.

Small amounts of water have been found in the heated holes in initial tests, the moisture presumably having been released from thin shale interbeds in the salt.

The experimental operations also include tests of equipment and methods for transporting and handling the radioactive fuel elements.

4. *Conclusions*.—The Committee is in complete agreement that (1) the use of caverns in salt beds as permanent storage sites for high-level radioactive solids has promise of being successful and satisfactory, and (2) that the tests now underway and scheduled in the mine at Lyons are well planned and should provide the data required for guidance of future operations. This experiment is now largely an engineering project. Our only suggestion regarding the geologic aspects is that ORNL include a study of the behavior of shale interbeds that will be subjected to long periods of elevated temperatures and radiation, in order to anticipate possible changes in wall, ceiling, and floor conformation additional to those caused by creep and flow of salt.

B. Grout injections in shale

1. *Background*.—The principal objective of this research and development project, first examined by the Committee in October 1958 and actively pursued at ORNL since then, is the disposal of intermediate-level waste liquids by mixing them in grout and injecting the mixture into artificially produced fractures in subsurface shale strata. The grout enters the fractures and spreads out in thin sheets, later solidifying in place.

As explained earlier in this report, the bedrock at Oak Ridge lies at or near the surface of the ground, and the shallowest permeable beds in it contain fresh water. The altitude of the ground surface here is about 800 feet above sea level, and in the disposal area the surface strata are part of the Conasauga Formation of Cambrian age. Directly under it is the Rome Sandstone, Lower Cambrian, which, owing to reverse faulting, lies on Chickamauga strata of Middle Ordovician age. All of the strata are folded and locally crumpled as well as faulted. The rock that was selected for experimental fracturing and injections is the Pumpkin Valley Shale Member in the basal part of the Conasauga Formation. At this locality it is about 300 feet thick and is first reached in the overthrust

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fault block at a depth of 700 to 1,000 feet below the surface. It is overlain by thin limestone beds and these in turn by gray shale.

2. *Engineering procedures*.—Preparation of a borehole for injection consists of drilling a hole through the injection formation, then lowering and cementing casing to the total depth. Prior to injection the casing is cut at the depth at which a fracture is desired, by directing against it, at high pressure, a jet of slurry consisting of sand in water. The horizontal slot thus cut in the casing and into the rock wall of the hole is thought to provide the initial orientation of the ensuing fracture.

Water is then pumped into the hole at pressures exceeding that of the combined weight of the overlying strata, with the result that the shale is fractured. Success of the operation depends in part on the creation of horizontal rather than vertical fractures, inasmuch as vertical fractures might lead upward into permeable strata and provide channels of escape through which radioactive grout fluid could contaminate potable ground water.

The grout mix is prepared at the surface, where all of the injection equipment is housed in concrete cells as a safety precaution. It is then pumped into the borehole under pressures sufficient to drive it through the slot in the casing and into the newly created fracture in the shale. The grout spreads through the fracture, forming a sheet which extends outward around the point of injection. It then sets up into a hard solid in a period of a few days.

The most recent series of injection experiments at ORNL consisted of five separate injections through one well, of both synthetic and real wastes, of various types of grout mixes, of dyes or radioactive tracers for subsequent identification of the grout sheets, of various fluid-loss additives, and of ion-exchange additives. The depths of the five injections, in the order of operation, were respectively 945, 924, 912, 900, and 890 feet. Breakdown pressures, at which the shale became fractured in each case, ranged from 2150 to 3800 p.s.i. The volumes of synthetic or real wastes mixed into grout and injected into the shale ranged from 27,300 gallons to 148,000 gallons per injection.

ORNL currently produces about 4,000,000 gallons of intermediate-level waste liquids per year. It is estimated that the evaporator which is now being constructed at the site will concentrate the wastes to a volume of about 400,000 gallons per year, and that this annual amount can be disposed of in two batches of 200,000 gallons each.

3. *Post-injection research*.—Monitor holes drilled around the injection well provide information on the extent of the grout sheets. It has been found that in some instances the sheet bifurcates, one or more times, resulting in multiple sheets from one injection. One explanation of this behavior is that the leading edge of the advancing grout becomes dehydrated, owing to absorption of water by the shale, with the result that increased viscosity creates a dam against which the more fluid grout cannot move; the fluid then breaks through to another bedding plane. An alternative explanation is that multiple fractures were produced by water under pressure and that the grout entered two or more fractures more or less simultaneously. Similarly, the grout sheet could divide at points of natural weakness in the formation, passing up or down along pre-existing fractures, and then propagating laterally along bedding planes. The field data suggest that all of these processes were operative in the ORNL experiments.

Further research involves measuring the amount of uplift of the surface of the ground resulting from insertion of several sheets of grout between beds of rock strata in the subsurface. The areal extent of each sheet is first determined in a general way by observing the arrival of the leading edge at predrilled monitor wells and also by drilling core holes in the area after the grout has set. The exact outline is impossible to obtain by this method, and it may be presumed that lobes or fingers extend beyond the mapped area in places and that indentations likewise occur. This would be true especially where the lithology of the reservoir formation is uneven so that travel of the grout is free in one direction or place and impeded in another.

The mapping shows that in many cases in the Oak Ridge experiments the outline of a grout sheet is ovoid rather than discoid, and that it is eccentric to the point of injection. The shape and accentricity are presumably due largely to structural attitude or dip of the beds, in which increasing overburden weight downdip forces the grout to move updip.

A series of traverses by precise spirit-leveling of the ground surface across the area of injections was conducted, in order to determine the amount of uplift caused by each injection. It was found that the amount of uplift is gen-

erally equivalent to the thickness of the group sheet as observed in cores taken from post-injection borings. The area of uplift, however, is not identical with the area of the sheet. The rock strata apparently reacted as a semi-rigid crust, so that uplift commonly extends beyond the observed edge of the sheet. As would be expected from such behavior, the outline of the uplifted area has no lobes or indentations. Although the seams of grout that can be observed in core samples of the strata are generally only about 1/8 to 1/4 inch thick, the maximum uplift of the surface is about 1/2 inch. As the maximum invariably occurs at or near the injection well, it is assumed that the grout sheet there is about 1/2 inch thick whereas farther away, at the cored locations, it is thinner.

The relations between volumes of waste and the amount of uplift are indicated by the following tabulation of two injections selected as examples:

Volume injected (gallons)	Area of grout (feet)	Diameter of uplift (feet)	Height of uplift at apex	
			Feet	Inches
97,000	700×250	1,000	0.014	0.168
132,000	650×400	1,500	.040	.480

The possibilities of damage that might be caused by repeated uplift of the ground surface around a single much-used injection well have been considered. As the amount of uplift increases cumulatively, presumably a position would eventually be reached at which structural damage would be caused to surface installations such as pipelines and buildings. Evaluation of this possibility is an engineering problem which the Committee is not prepared to discuss.

Another possible type of damage, more serious but also more difficult to appraise quantitatively in advance of its occurrence, is contamination of potable ground waters above the grout sheets when tension cracks, small faults, or other fractures are created in near-surface strata by stresses resulting from arching. This kind of damage is not probable, however, for in a vertical sequence of 900 feet of layered rock the distortion would probably be created by elongation, which presumably would result in normal faulting; the slippage would be of small displacement, leaving no open fractures. In contrast, the amount of vertical uplift created by the injections is trivial, where 100 injections in the same well would raise the surface only 50 inches.

4. *Conclusions.*—Hydraulic fracturing of rock formations is widely practiced throughout the petroleum industry. Experience gained through this work has indicated that fracture orientation depends on naturally inherent local stress distribution and rock inhomogeneities which vary from one geologic environment to another. We now know that general statements regarding fracture behavior can be misleading. For example, although it was early recognized that bottomhole pressures equal to 0.6 or 0.7 times the overburden pressure would always produce vertical fractures, it was also held that horizontal fractures would always be formed if overburden pressures were exceeded in the well bore. Experience has shown that the majority of all induced fractures are vertical: there is probably little horizontal fracturing at depths greater than 1000 feet, and 50 percent of shallow fractures are vertical. Inasmuch as vertical joints produced by natural forces commonly occur even in areas of tectonic stability, the prevalence of vertical induced fractures must be anticipated. Each case and area must be evaluated individually in terms of rock characteristics and pressures required to produce various types of fractures.

A few examples of fracture-pressure relations in the field illustrate the preceding comments. At depths of 1300 to 1500 feet in the Anona Chalk, Pine Island oil field, Louisana, only vertical fractures were produced at pressures exceeding overburden pressure. In Colorado, the Green River shale yielded only vertical fractures at depths of 100 to 200 feet and at pressures two to ten times the overburden pressure. Horizontal fractures were produced at 400 feet in the Athabasca tar sands in Alberta at two to three times the overburden pressure, but the fractures did not coincide with notches that were cut in the well bore. Thus, in any given field situation the formation of interest should be subjected to experimental fracturing in order to determine how it will respond to various bottomhole pressures. As apparently is the case at ORNL, certain formations with characteristic bedding-plane weaknesses will be amenable to horizontal fracturing at shallow depths. At the same time, however, some vertical fracturing should also occur, especially where pre-existing joints are present.

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It must be emphasized, therefore, that the successful experiments at Oak Ridge are probably not duplicatable at all other sites, because of differences in geologic conditions from place to place. Local conditions should be investigated carefully and pilot tests conducted before a site and a shale formation are accepted for grout injection.

In the routine of monitoring the grout injections, continuous liaison should be maintained between waste-disposal personnel and the specialists at major industrial research laboratories where new subsurface engineering techniques are being developed. Technological advances in downhole logging and reservoir mechanics, such as the use of cement-bond logs and fracture-finding devices, should be exploited in subsurface disposal procedures. The use of tiltmeters to record minute changes in surface levels will help to map the distribution of grout sheets. Disposal technologists may well profit from early knowledge of new subsurface devices and methods.

C. Solidification of liquid waste

Several processes for converting high-level liquid wastes to solids have been investigated, and some have reached an advanced stage of development. Basically, the products of these processes fall into two categories, the calcines and the glasses or ceramics. These processes which have shown sufficient promise to warrant more extensive development are: pot calcination, fluidized-bed calcination, spray calcination, and the introduction of phosphate, borate, or silica to yield a glassy product that is resistant to leaching. Currently a Waste-Solidification Engineering Prototype is under construction at Battelle Northwest Laboratories (formerly Hanford Laboratories) where some of these processes will be tested under operating conditions with actual high-level radioactive wastes. To be demonstrated are the pot and spray calcination processes and the production of phosphate glass and ceramics. Under present plans the solid products will be stored at the surface in stainless-steel tanks enclosed in concrete shells, and will be studied in order to determine their long-term stability.

The ultimate objective of the entire solidification program for high-level wastes is the production of a form of waste which can be stored safely, economically, and with the greatest security to mankind. The engineering aspects of the processes are not within the scope of this Committee's ability to evaluate, but the manner of disposal or storage of the end products is of concern to us to the extent that it involves the geologic environment. In this respect we are interested also in the stability of the product.

The Committee is favorably impressed with the whole solidification program, because it promises relief from the problems of storing and disposing of high-level liquid wastes at present sites where disposal into the ground anywhere near the fresh-water aquifers would be intolerable. We are especially hopeful about the glass or ceramic products, because they may be safe from serious leaching and, thus, from release of hazardous radionuclides, for periods of many centuries.

At the same time, the Committee does not favor the suggestion that high-level radioactive calcines may safely be stored above ground or at shallow depths and above fresh-water aquifers at existing plant sites as a permanent disposal. In our opinion such a disposal of high-level long-lived radioactive solids is contrary to the rules of safety which AEC has sought to establish for handling of radioactive materials. The emplacement of such hazardous solids in salt vaults after a suitable cooling period would appear to be an obvious requirement.

An alternate suggestion that high-level radioactive solids might safely be stored in tunnels constructed in hillsides in a desert environment cannot be accepted without thorough research and experimentation. Of particular concern here would be the travel of ground-water floods resulting from once-a-century catastrophic deluges.

D. Ion-exchange studies

1. *General.*—Ion exchange and other sorption phenomena have been studied by research personnel at the various AEC installations with respect to both synthetic materials and natural minerals. The studies include not only evaluation of the usefulness of synthetic materials and natural ion-exchange minerals for predisposal waste-treatment processing, but also the determination of the ion-exchange characteristics of the earth materials underlying the AEC reserva-

tions and environs. The earth materials are best studied *in situ* rather than in the laboratory, because of directional phenomena resulting from lithologic variations from place to place. The studies concerned with waste-treatment processing reveal economic parameters which compare favorably with those of other forms of fixation that are being considered for predisposal processing of waste.

We concur with the investigators that ion-exchange materials may be useful as a means of removing radionuclides from low and intermediate-level wastes in order to permit waste water to be disposed to the environment. By ion exchange the radioactivity in high-level wastes also is fixed in a form for easy packaging as a recovery procedure in order to render the isotopes available for subsequent beneficial use. It should be noted that efficient sorbants that are completely or nearly saturated with radionuclides must be considered as intermediate level waste and treated as such during ultimate storage or disposal.

In connection with direct disposals to the ground, it is emphasized that (1) where the exchange capacity of the soil or rock is high, constant watchfulness will be necessary in order to avoid excessive concentrations of radioactivity, and that the distribution pattern of the waste will not be known exactly; (2) where the exchange capacity is not high, channelization will take place, as in the basalt at NRTS. In either case future changes in groundwater composition or patterns of flow may desorb the radionuclides and move them—an intolerable risk, or they will eventually proceed at excessive levels directly to the environment.

Ion-exchange studies of the earth materials underlying the four operating sites are in progress, and the Committee agrees with the investigators that in some areas much remains to be done. At no sites have studies been completed on the distribution and exchange capacities of the reactive materials in the soils and the underlying consolidated or unconsolidated rocks, and thus there is not a full understanding of the sorption and fixatiton process and rates of movement of radionuclides beneath the plant sites.

2. *Savannah River Plant.*—The Savannah River Plant is located in a humid region having a complex drainage network tributary to the adjacent Savannah River. The site is underlain by about 1000 feet of partially consolidated sediments. Because much of the surficial material is sandy and admixed with kaolinitic soil or clay lenses, ion exchange can be over-emphasized as an important retardant of radionuclides at this site. Low-level wastes have been discharged to pits and streams. Abandoned pits have been isolated hydrologically by clay caps and peripheral dikes. Radioactively contaminated solid trash is buried in shallow trenches about 20 feet deep under a soil cover. High-level wastes are neutralized and stored in steel tanks which are buried under 8 or 10 feet of soil. Several factors point to a potentially hazardous problem resulting from past and present disposal practices. Long-term hydrologic studies of the ground-water regimen at Savannah River Plant have not been made; therefore, predictions of the maximum (once-in-century) rise of the water table would be tenuous. At this site, with its shallow water table, an exceptional rise of the water table could easily cause invasion of the abandoned pits and trash-containing trenches. Radionuclides would then be subject to desorption and other forms of leaching that would permit them to be mobilized and carried into the regional ground-water system. A major failure of one of the high-level tanks would probably overwhelm the safety devices designed to cope only with small seeps or leaks.

3. *Oak Ridge National Laboratory.*—At ORNL much research is in progress in the field of mineral sorption; emphasis currently is on "fixation" (irreversible) phenomenology as compared to ordinary ion-exchange processes. The value of these studies will be proven if an economical process is developed to remove large quantities of radionuclides from liquid wastes by fixation on material that can be safely and economically stored or disposed. Unfortunately, most of the natural minerals that are able to "fix" radionuclides commonly found in waste are uncommon in the earth materials underlying the ORNL site. However, it has been reported by ORNL that desorption of cesium from Clinch River sediments is so low under most conditions that it may be considered "fixed." Past pit disposals, unscheduled releases, and leaks from stored or transported material have saturated the ion-exchange capacity of some of the sediments and rocks for certain radionuclides in parts of the ORNL site. Ground water that is tributary to the Clinch River or to its tributary streams commonly contains substantial quantities of strontium[90] and rubidium[106]. It was reported to the Committee

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that in the reach of the Clinch River between ORNL and its confluence with the Tennessee River, radioactivity measurements indicated that the gross radioactive burden of the bottom sediments was 2000 curies. Most of this activity is, in all probability, derived from radionuclides sorbed on the sediments. However, it is not known what fraction is actually adsorbed and what is immobilized by being held to particulate matter by some other mechanism.

Because ORNL, with a climatic and geohydrologic environment similar to that of the Savannah River Plant, has little in its environment to attenuate radionuclides except the ion-exchange capabilities of the system, here too disposal of waste to the environment should be discouraged. Limited ion-exchange capacity and hydrogeologic characteristics at these plants prescribe that much of the environmental disposal capacity should be held in reserve as insurance for operational accidents or possible accelerated movements of radionuclides from waste previously disposed. It would appear that a systems analysis of the ORNL site, the Clinch River, and part of the Tennessee River is called for, in order to determine the waste budget of the entire system. In this way, the impact of radioactive waste on the biosphere from both the stationary ion-exchange media (earth materials at the site) and the mobile ion-exchange media (stream-bed sediments) may be appraised.

4. *National Reactor Testing Station.*—The National Reactor Testing Station, Idaho, is underlain by basalt flows which include intercalated sediments and in large areas a surficial cover of alluvium. Some liquid wastes are released to the environment at the site through wells or into ponds. Radioactive trash and other low-level solid material are buried in slit trenches covered by several feet of back fill. Gross ion exchange beneath the site is thought to be small not only because of the limited amount of sedimentary material, both surficial and intercalated, but also because the basalt has been little altered to minerals having ion-exchange capabilities. Although some ground water is perched in both the sediments and the basalt, the top surface of the regional groundwater body lies entirely deeper in the basalt, the water table lying several hundred feet beneath the land surface. Thus, with transmissibilities in the basalt as high as 15 million gallons per day per foot and a maximum rate of travel time of 50 to 60 years to springs in the Snake River Canyon, it is imperative that dilution and decay of radioactive wastes be considered when evaluating the safety of ground disposal at this site. Storage basins using clinoptilolite or other ion-exchange material seemingly are very effective for removing radiocesium and slightly less so for removing radiostrontium to less than drinking-water standards. It would seem that this method should be encouraged for primary treatment of wastes and that the ion-exchange capacity of the earth materials should be reserved for emergency safety procedures.

5. *Hanford Atomic Products Operation.*—The Hanford Atomic Products Operation is making studies for the use of ion-exchange both as part of its waste-processing practice and as a useful natural radionuclide retainer in the subsurface material at the site. Additional waste is now being disposed of directly to the ground. Most of this waste is discharged to and contained in the sedimentary Ringold Formation which overlies the basalt terrane in this area. The movement of the waste has been down gradient towards the Columbia River, but with a laterally spreading dispersion front. Careful monitoring of the movement of this waste indicates that there has been some differential retardation of the various radionuclides by ion-exchange; tritium and ruthenium seem to have been the least attenuated. A problem may occur when this waste front breaks through at the river channel. After break-through, continued disposal to the Ringold Formation would sustain the volume of effluent to the river, but cessation of waste disposal to the ground would not necessarily alleviate immediately the problem of radioelement contamination of the Columbia River. The Ringold Formation could drain for years after ground-disposal practices have been stopped, but probably with a tapering-off of the volume of effluent. Any contributions of radionuclides to the Columbia River from subsurface effluents will have to be considered as part of the river burden when determining the one-tenth RCG at the nearest point of use. Should direct discharge of waste to the Columbia River be at or near the concentration restrictions that are determined by the downstream use, plant operations may have to be curtailed or other means found for disposal of the waste.

6. *Conclusions.*—At all sites where continuous disposal of low-level wastes or frequent unscheduled releases to the earth materials underlying the site

occur, there is always the danger of a build-up of concentrations in the soil and underlying rocks. An equilibrium may be established by balancing the rate of disposals with the rate of decay of radioactivity; continuous disposals beyond this rate could lead eventually to hazardous excesses of concentration, at which point the earth materials would no longer be a suitable disposal medium.

Future changes in the ground-water regimen through intensive agricultural irrigation or nearby construction of dams or other water-regulatory works may easily affect earth materials containing adsorbed radionuclides. Especially active will be the unconsumed irrigation water containing dissolved fertilizer components and biological refuse from plants and soils. Since ion-exchanged nuclides on earth materials are subject to reversible equilibria, under leaching conditions that may be superimposed by radically different water introduced into the system, there may easily be induced a removal rate that is much faster than the sorption rate. The future chemical condition of the soil and applied water is not always predictable, as man's requirements from his environment are likely to change in future years of land use in the United States.

E. Bedrock storage at Savannah River

1. The waste disposal problem.—High-level liquid wastes at SRP are alkaline, aqueous solutions derived from the treatment of spent reactor fuels in chemical-separation plants. They are piped first into water-cooled tanks to remain there until the greater amount of heat from decaying radioactivity has been dissipated. Low-activity water is then evaporated and the condensate sent to seepage basins; the residue goes into uncooled storage tanks. The tanks, built of steel and concrete, are located below the surface of the ground, each one on a steel "saucer" which serves to collect small quantities of liquid in case of a leak in the tank. At present there are 24 tanks having a total capacity of 23,400,000 gallons.

After about eight years of storage for decay of the shorter-lived isotopes, a typical waste contains several inorganic sodium compounds, 0.1 to 10 curies per gallon each of strontium^{90} and cesium^{137}, and trace amounts of plutonium and neptunium. Physically it consists of two fractions—a supernatant liquid containing nearly all of the cesium and a sludge containing nearly all of the strontium, probably as an insoluble carbonate. Inasmuch as laboratory tests indicate that reaction between either an alkaline or an acid waste liquid and the rock of the basement complex would form a nearly neutral solution, it is assumed that after several years storage in a bedrock chamber the strontium would be in solution as a bicarbonate.

Strontium^{90}, with a 28-year half-life, and cesium^{137} with a 30-year half-life, are the elements of hazard in these wastes, for a period of at least 600 years would be required for their decay to innocuous levels. Although no severe emergency has yet appeared in the tank-storage program, four lesser leaks have occurred in a little over ten years, and it is commonly agreed that safer permanent storage or disposal facilities are demanded. The concept of emplacement in caverns to be excavated in the basement rock under the plant site is currently a proposed solution to the problem. The engineering design would include the construction of a shaft through the sedimentary column and far enough into the basement rock to be clear of the upper interval of more highly fractured rock. Investigations to determine the feasibility of the method are being conducted by SRP personnel.

The critical factor in all of the investigations and in the Committee's discussions is the proximity of the basement rocks to the fresh-water aquifers which overlie them, the only separation between the two being approximately 50 to 100 feet of clay. Pertinent aspects of the geology of the proposed disposal site are described more fully in the following paragraphs as a prelude to further discussion.

2. *Geology.*—Sedimentary strata that have been penetrated in various borings at the site have a combined thickness ranging from 877 to 972 feet. All permeable beds in this stratigraphic section contain fresh water in various amounts and of various qualities; in all of them the water is potable. The Tuscaloose Formation at the base of the section contains two aquifers separated by a layer of relatively impervious clay about 60 feet thick. The thickness of the upper aquifer is about 170 to 250 feet, the lower one about 200 to 270 feet. Large supplies of soft water, low in dissolved solids, are present in both Tuscaloosa aquifers; in fact these are the principal reserves of fresh water upon which the population

of much of South Carolina and adjacent parts of Georgia probably will depend in the decades of the immediate future.

The clay aquiclude at the base of the Tuscaloosa Formation has a thickness in the range of about 20 to 40 feet. Beneath it is a layer of saprolite clay about 30 to 60 feet thick, lying directly on the crystalline rocks of the Precambrian basement. The average thickness of the two combined clay layers is about 70 feet. The saprolite is thought to be the product of weathering of the Precambrian surface rocks before the time of Tuscaloosa deposition, and presumably is a residual deposit; that it, it was essentially still in place when covered by the lowest member of the Tuscaloosa Formation as advancing seas submerged the area in late Cretaceous time. However, it is reasonable to suppose that some agitation, transportation, and then deposition of the surficial materials occurred as the shoreline moved inland across the area.

In actuality the thickness of the saprolite layer is probably not susceptible to precise measurement, as the boundary at the base is no doubt gradational. Proceeding downward, the lithology may be expected to pass progressively from "pure" saprolite clay into clay containing fragments of crystalline rock, into a zone of boulders separated by smaller rock fragments in a clay matrix, into "rotten" rock containing lesser and lesser admixtures of clay, and finally into more or less unweathered bedrock. A large boulder surrounded by products of decomposition, and being essentially a bedrock segment enclosed between vertical and horizontal joints in which decay had been accelerated by circulating meteoric water, would probably be logged as the base of the saprolite clay in drilling.

During its exposures to the atmosphere in Mesozoic time, the basement surface was a part of the seaward sloping coastal plain, across which flowed the streams that had their sources in the mountains to the northwest. The miscellaneous character of the bedrock probably led to the development of a dendritic drainage pattern very much like that in the present Piedmont province. The flow of the major streams maintained channels in which residual soils were not allowed to form, but which instead were filled by stream deposits such as sand and silt. Identification of such deposits in the base of the Tuscaloosa Formation by borehole data alone is difficult; for that matter, penetration of a channel deposit by an exploratory borehole would be largely fortuitous in the light of currently available evidence.

The topography of the interstream areas must have been similar to that of the present Piedmont province, uneven and hilly, and the crystalline rocks undoubtedly were exposed at the surface in some of the more prominent hills as well as in stream-cut banks. It is difficult to imagine an area of Precambrian outcrop in which a soil cover blankets the entire surface without a break. Recognition of the probable existence of buried channels and erosional knobs beneath the Tuscaloosa Formation in the coastal plain is critical to any interpretation of subsurface data regarding lateral continuity of the layer of saprolite clay.

The basement rock consists of gneiss and schist, both of which are foliated crystalline rocks, and lesser amounts of quartzite and phyllite. The upper portion of the rock complex is generally highly fractured and weathered, the degree of fracturing and decomposition becoming less with greater depths. Below the upper 200 or 300 feet the rock is generally more nearly whole, although fractures do persist to the greatest depths that have been observed. Some fractures are "hairline" cracks which can be detected in hand specimens only by the application of penetrating oils. Others may be a measurable fraction of a millimeter. In the case of faults, where the rock mass on one sio has moved in relation to the rock on the opposite wall, they may be inches wide and filled with fracture rubble. No such fault-gouge or breccia zones have been recognized in the deep boreholes thus far at SRP.

Obviously the rate of flow of ground water through the fracture system should be expected to vary from place to place, depending on the width of the cracks and crevices, but over large areas a single rate may be computed. It is the hydrology of this flow system that SRP has endeavored to analyze by tests and observations in deep boreholes. Seven holes have been drilled to depths approaching 1000 feet below the top of the crystalline basement rocks at the proposed excavation site. An eighth hole, near the Savannah River in the west corner of the SRP reservation, reached the basement at a depth of 690 feet and was completed at a total depth of 765 feet. A ninth, in the south part of the reservation, reached the base of the Tuscaloosa(?) Formation at 1218 feet; a

core taken from the interval 1299 to 1313 feet, the total depth, is rock that has been identified as Triassic in age.

The southeastward or seaward slope of the pre-Cretaceous surface is revealed by the increasing depth of the base of the Tuscaloosa sandstone in that direction. The well logs also indicate increasing thickness of the Tuscaloosa Formation itself in the *same direction*, although exact stratigraphic correlation of the sand stone units above the Triassic sedimentary rock in the ninth hole has not been determined. Ground-water reserves are therefore assumed to increase southeastward toward indefinite boundaries where the aquifers, becoming deeper, *are more* compacted and less permeable, the sandstone becomes more shaley, and the water is more saline.

3. *Hydrology.—(a) Present status of investigation.*—A great mass of information has been accumulated concerning both the nature of the rocks and the hydraulic characteristics of the water systems in the sedimentary strata and in the underlying basement. In addition to the nine deep wells mentioned above, nine shallower borings have been drilled, in clusters of three at three different locations in the prospect area, as observation wells at various depths in the Tuscaloosa Formation. Data regarding water levels and their fluctuations have been recorded from all eighteen observation wells in addition to shallower water wells that are producing in the area, and estimates of permeability and flow rates in various zones have been made by computations from input and output volumes in pumping tests. Samples of fluids and rocks in the Tuscaloosa Formation and the basement complex have been analyzed for chemical and physical characteristics. As would be expected, fluid movements were found to be much slower *in zones of* essentially "unfractured" basement rock than at depths where conspicuous major fractures had been logged. At the same time, withdrawals from major fracture zones in one well affected water levels in other wells, indicating that the network of fractures contains a single hydrologic system throughout the observed portion of the basement.

Several groups of data, computations, and lines of apparently related evidence lead SRP to conclude that the hydrologic system in the basement rock is effectively separated from that in the Tuscaloosa Formation by the *saprolite clay* layer:

(1) The clay layer is essentially impermeable or only slightly permeable and is believed by the on-site investigators to be continuous and unbroken throughout the area of the SRP reservation.

(2) The hydraulic head in the basement aquifer stands about 20 feet higher than that in the Tuscaloosa Formation.

(3) Both chemical analyses and electric conductivity measurements show a greater content of dissolved solids in the basement waters (822 to 7140 ppm; 1250 to 9650 micromhos) than in the Tuscaloosa (39 to 65 ppm; 55 to 100 micromhos).

(4) Chemical analyses show different gas contents in the two aquifers:

	Total gas ml/l	Average composition (percent of volume)						
		N	He	A	O	H	CO	CO_2
Tuscaloosa	0.1	93.2	0	1.09	4.14			1.61
Basement	.6	91.0	6.0	1.1	1.1	0.3	.04	.2

Mapping of fractures in the basement, and calculations of the rates of water movements in both systems, coupled with the interpretations of the saprolite clay layer as an effective seal between the two, led to the following conclusions by the SRP staff regarding the safety of the proposed storage procedures.

(1) By the aid of the perliminary borings at a selected site for excavation it should be possible to place a storage chamber in a location that is *not crossed* by a major fracture.

(2) By only partially filling a storage chamber with waste liquid and then sealing it and thus creating locally a hydrodynamic low-pressure anomaly, the influence of the natural hydraulic *gradient as a mechanism* in moving the radioactive fluid out of the chamber would be delayed. In view of the slow movement of fluids in the "sound" rock in which the chamber is to be constructed, it is estimated that a delay of perhaps 100 years can be effected.

(3) Having escaped into fine fissures in the "sound" rock, the radioactive fluid will move at a rate which is calculated to be about 1.4 feet per year, or 840 feet in 600 years. At this rate, travel time to the Savannah River would be 30,000 years.

(4) If in its travel the radioactive fluid finds and enters a major fracture, its velocity then becomes something on the order of 7.2 feet per year or 4320 feet in 600 years. Travel time to the Savannah River would be 5,500 years.

(5) If escape routes should lead upward into the Tuscaloosa Formation through an unsuspected path in the saprolite clay layer, the fluid would move downgradient in the Tuscaloosa at a rate computed to be 360 feet per year or a travel time of 100 years to the Savannah River.

(6) The above computations disregard the effects of ion-exchange, which would be relatively insignificant in unaltered basement rock, moderately effective in weathered rock, and substantial in the Tuscaloosa Formation. Laboratory studies have been made to determine the ion-exchange characteristics of the various rock types in the Tuscaloosa Formation and the Precambrian basement.

SRP concludes that the delaying actions of the several "barriers" cited above are sufficient to make the proposed disposal method safe. The long travel times would render *the radionuclides ineffective* as hazards by the time the Savannah River is reached, and numerous impermeable barriers above the Tuscaloosa aquifers would protect surface drainage from contamination by fluids that might escape from the basement storage chambers.

Accordingly, a conceptual design has been developed for excavating a storage facility in the basement rock. It would consist of four 5,000,000-gallon storage chambers 1300 to 1700 feet underground, an access shaft, service facilities, and encased pipelines from the Separations Areas. The construction cost is estimated to be $12,500,000, a unit cost of $0.625 per gallon of stored waste. If the chambers are filled to only two-thirds of capacity, in order to create the negative hydrodynamic anomaly that has been suggested, the unit cost would be $0.938 per gallon.

(b) *Evaluation of present hydraulic information.*—Much ingenuity in testing the systems and a great deal of thought in interpreting them have been demonstrated by the SRP staff. However, in the absence of readily available other methods, computations of *transmissibility and of velocity of movement in the* crystalline rock of the basement have been based on concepts which originated and were developed for homogeneous sandy formations ("granular aquifers"). Many anomalies in the reactions of the wells to pumping, earth tides, and atmospheric *changes show that the concept is of doubtful validity in* this application. Further, the concept for computation of velocity of the water in the fractured crystalline rock is based as an essential part on the potentiometric gradient between the prospective site of the shaft and the Savannah River. Knowledge of the water level in the Precambrian bedrock at the river is based on the water level in one well, in which it can be determined only with considerable uncertainty. The potentiometric map for the bedrock water, based in part on this water level, represents an unexplainable pattern of flow. In any future work all of the data that were used in constructing this map should be evaluated anew in the light of the overall pattern of flow that is implied.

With regard to the pumping test data, the use of the Jacob method by the SRP staff, in which only the constant and log terms in the expansion of the exponential integral or "well function" are used, is not justified because for these data the other terms in the expansion are significant. However, the inaccuracies in the computation of transmissivity and storage do not appear to be serious *in the present context*. More seriously, *the given* data do not conform in detail to the theory of cylindrical flow in a quasi-homogeneous aquifer. In most wells there was a different rate of response in the early stages of pumping of DRB 6 than in the later stages.

All data from the pumping tests seem to indicate that the hydrologic system in the basement rock in the test area is completely or almost completely isolated from that in the overlying Tuscaloosa aquifer; the drawdown curves for the wells increase with time in rate of fall, relative to that required for cylindrical flow, instead of flattening as they should if vertical replenishment through the saprolite were taking place. However, so long as the characteristics of flow are not completely understood some elements of doubt remain.

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Five types of information are available for the combined rock-water system in the basement:

(1) A model to serve as a criterion may be set up as a flow through a series of equally spaced plane fractures to account for the transmissivities observed during the pumping tests. Using values of transmissivities and thicknesses of aquifer that are acceptable for the data of the pumping test, the width of such a fracture, in centimeters, is in the order of 10-3 s⅓, in which s is the spacing of such fractures in centimeters. The porosity of such a system is 10-3 x (⅛) ⅔. These data indicate fracture widths and porosity, for fracture spacings as follows:

Distance between fractures (centimeters)	Width of fracture (millimeters)	Porosity (percent)
1	0.01	0.001
30	.03	10^{-4}
300	.067	2×10^{-5}
3,000	.173	5×10^{-6}

These fantastically small widths must be increased somewhat for (a) increased length of path between points through an intersecting joint system, (b) irregular and probably rough walls in the actual fractures, (c) motion around places of contact of the walls, (d) perhaps electrical effects on the moving dissolved ions. The previously indicated porosities would be increased proportionately. Nevertheless it seems probable that the actual velocity of the water in the cracks in the crystalline rock would be much greater than that computed by the SRP staff because these small porosities apply to the "major fracture zones." A very interesting problem is also posed as to ion-exchange characteristics of such very small cracks, perhaps with mineralogically somewhat altered walls.

(2) With the reduction in head and pressure in the water in the fracture system during pumping, an equal pressure from the weight of the overburden is transferred to the rock system, and the strain pattern in the rock is changed. Some water is taken from storage owing to the expansion of the water and any change in the volume of rock that may occur owing to the shift in stress distribution. In hydrologic work with granular aquifers it is usually assumed that the overall thickness of the aquifer is changed by the shift in stress, and that the total volume of the sand grains is not changed. A fairly simple model results. In fractured crystalline rocks of very low overall porosity, the contribution of the expansion of the water to the "coefficient of storage" of the hydrologist, or to the "compressibility of the water and formation" of the oil-reservoir engineer, is comparatively small, and the deformation of the rock masses between fractures may be of great importance. This model is apparently more complicated.

(3) An increase in barometric pressure depresses water levels in wells. If none of the pressure was impressed on the water in the aquifer, the drop in water level would be equal to the rise in a water-barometer. The difference between the two represents the part of the barometic load that is imposed through the overburden on the water in the aquifer. In the case of a granular, confined aquifer, there is again a simple relation between the "coefficient of storage" and the "barometric efficiency" of a well, and one can be computed from the other, at least approximately. Again the model for a fractured crystalline rock may be more complex, but a proper interpretation may further indicate the characteristics of the fractures. Apparently about one-half of the barometric variation is transferred to the water in the rock system in the Savannah River wells, although apparently also there are some lag effects that may suggest other characteristics of the fracture system.

(4) The body tide of the earth produces a small strain in the rocks near the surface of the earth amounting to the order of 10^{-7} on the horizontal cross section. This tiny strain produces in all wells in confined aquifers a measurable fluctuation, in many wells concealed by larger fluctuations from other causes. In the wells that are in good connection with the fracture system in the Savannah River area (all but deep wells DRB-2 and DRB-7), the maximum fluctuation, at times of new and full moon, is about 0.3 feet. Because the tide is a cyclical phenomenon those wells that are in poor connection with the joint system should theoretically show fluctuations with decreased amplitude and a lag in phase. DRB-2 has a maximum fluctuation of about .03 feet and seems to be about four hours out of phase. DRB-7 has a still smaller fluctuation, and its phase lag is difficult to determine.

The tides produce a definite small strain in the aquifer as against a stress due to barometric loading and a shift in stress from water to rock material during pumping tests. It is possible that an analysis of the three effects will give a clue to both the size and the distribution of the fractures in the main water-transmitting joint system.

A very peculiar perturbation of water level occurred during the pumping of wells DRB-3 and DRB-6 in the two tests involving pumping of these wells. Ostensibly these fluctuations are periodic, suggesting earth tides and probably barometric effects, but if so the effects are greatly magnified in the pumping wells, apparently amounting to a few feet in DRB-3 and ten feet or more in DRB-6. Unfortunately changes in the pumping rate were made during both tests and, especially in DRB-6, measurements of pumping rate and depth to water were made at erratic times during the day. However, the fluctuations do not correlate with the recorded data on pumping rate and water level. If such large fluctuations are indeed to be correlated with the deformation of the rock system by tidal and barometric change, apparently they can only be caused by a change in fracture width and consequently in permeability. The observed changes could be so explained in at least a simple model if the main fractures are spaced something in the order of 100 feet apart.

(5) The tracer test with tritium, for which data are not now available, should provide among other things an independent estimate of the total volume of fracture space between the two wells. Thorough investigation of the hydrologic system in the basement complex by tracer injections of tritium should be of great value in revealing the direction; extent, and rates of movement of fluids. Full use of this technique is highly desirable.

(c) *Recommendations for further hydrologic research.*—The combinations of all the above forms of data should indicate a consistent model for the hydraulic characteristics of the basement complex, and therefore the nature of the rocks and the fractures in them. Until such an analysis is made it will be difficult to know the sort of hydrology with which we have to deal.

In order to make the hydraulic data more meaningful, two things should be done. Continuous water-level recorders should be installed on all wells, including those with small piezometer tubes and the two deep piezometers at a distance from the closely drilled site. A microbarograph or other means of maintaining a continuous record of barometric fluctuations should be installed at the main drilling site. The regimen of barometric response and of tidal response should be determined for all wells and should be of primary importance in learning the nature of fracturing at all available test points.

The second thing to be done is to make another pumping test under carefully controlled conditions with great care to maintain the rate of flow within an accuracy of one percent. Readings of water level should be made in the pumping well at fixed times of the day with assurance that no errors occur. The test should be run at least through periods between full moon and new moon, and at these times readings of water level in the pumping well and flow should be made around the clock. Evidently a continuous recorder would be preferred.

In the previous tests, anomalies in the data were treated as nuisances to be disregarded in fitting the data to the concept of a granular aquifer. Another test should be made in search of anomalies that are due to the nature of fractured rock aquifers, and no anomalies that are due to faulty procedure can be tolerated. Wells can be used as geophysical instruments, and the fluctuations of water in them under known conditions are likely after adequate study to give a truer picture of the hydraulic system than do any of the conventional geophysical techniques.

4. *Conclusions.*—The Committee recognizes with appreciation the intensive and intelligent work of the SRP staff on the problems connected with the bed-rock-storage concept, but is still dubious about its demonstrated safety. The placement of high-level wastes 500 or 1000 feet below a very prolific and much-used aquifer is in its essence dangerous and will certainly lead to public controversy. Any demonstration of its safety must leave no shadow of doubt.

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The Committee is divided with respect to the advisability of continuing the investigations toward bedrock disposal of high-level wastes at SRP. Those who favor continuance feel that the next steps should include:

(1) Re-analysis of all the hydrologic data from the basement rocks on the basis of fracture porosity rather than by methods designed for analyses of granular aquifers.

(2) If the new analyses provide sufficient justification, the performance of new and more carefully controlled tests in existing wells as described elsewhere in this report.

(3) If analyses of data from the new pumping tests are encouraging, the drilling of several additional deep observation wells as locations where the excavation of shafts and vaults might be contemplated. At least four borings into the basement at any such location should be planned. Presumably the new drillsites would be chosen where the least permeability in the basement rocks might be expected, as perhaps in the vicinity of DRB-2 and DRB-7, and eastward. The hydraulic characteristics of the rock-water system would be studies in these wells in the ways outlined in Chapter VI-E.

In drilling any new borings the shale and clay interval at the Mesozoic-Precambrian contact should be sampled and studied, particularly with respect to dissolved elements in extracted fluids at progressive levels in the interval. Also, sonic (acoustic) logs of the complete borings, from top to bottom, should be made at each drillsite, for use in evaluating the possibilities for a successful seismographic survey of the basal clay layer.

The value of tritium-tracer tests for empirical determinations of flow direction and velocity in the basement rock is emphasized again because of their immediate usefulness and accuracy.

The excavation of a shaft and chambers would provide the best evidence of the possibility of permanently leakproof chambers. The Committee reiterates its recommendation of earlier years that SRP representatives visit one or more mines in Precambrian basement rocks in the Piedmont province, in order to observe the water-entry problems in those places. It probably is pertinent also to point out that although the integrity of a mined opening as a leakproof chamber can be examined and evaluated, the construction of a shaft and chambers is not expected to add substantially to knowledge of the hydrologic conditions in the basement rock in sectors not contiguous to the excavations.

It is the opinion of the majority of the Committee that the prospect of storing high-level nuclear wastes safely in the crystalline rocks below the prolific Tuscaloosa aquifer is poor. Mining experience indicates that the amount of water developed in mines is much greater than that produced by wells in the same rocks, because mines intersect a much greater number of fractures and other water-bearing zones than wells of comparatively small diameter are likely to do.

There are several elements of indecisiveness in the present evidence for or against the safety of the proposed storage system. In the first place, rapid growth in demand for water can well lead within much less than 600 years to the need for heavy withdrawals of water from both Tuscaloosa aquifers.

In the second place, work on the hydrology of the bedrock has been based largely on methods developed under the concept of the homogeneous granular aquifer. Many anomalies in the hydrologic data thus far obtained confirm the *a priori* doubt that such a model can be trusted when applied to fractured crystalline rocks.

Thirdly, any slow leakage from the bedrock system into the much greater flux in the Tuscaloosa Formation would almost-certainly be unrecognizable by chemical means. The difference in helium content between the two systems, for example, may be due to the inability of analytical methods to detect minute amounts of helium that might enter from the basement rock into the Tuscaloosa Formation, there to be promptly swept downstream. A similar observation may be made with respect to the differences in salinity. The greater amounts of nitrogen, oxygen, and carbon dioxide in the Tuscaloosa water may simply reflect a greater intake of air in recharge areas.

Fourthly, the difference of only 20 feet in hydraulic heads between the systems above and below the saprolite clay layer at the observation wells is insufficient evidence to prove complete effectiveness of the clay barrier throughout the area of basement rock which could become contaminated by escaped radioactivity. The hydrologic data, if correctly interpreted, reveal conditions at and near the test borings, but at short distances down-gradient, the direction in which contamination would move, different conditions may exist. There may even be an area in which no clay layer separates the two aquifers. A widespread grid of observation wells would be required to map the hydrologic systems to the extent required for complete safety.

A much more rapid and less expensive mapping technique exists and might aid in establishing the continuity of the clay layer. Currently available data are insufficient for determining its usefulness in this application, but the needed data can be obtained during any future drilling operations. The technique is high-frequency seismographic exploration, a geophysical tool which has been used successfully by the petroleum industry (See Chapter VI-G, "Geophysical studies at NRTS"). It consists of measuring the depths to lithologic interfaces between rock layers of contrasting acoustic velocity characteristics by means of a high-frequency acoustic signal that is emitted from a source at the surface or in a shallow borehole. The time interval that is required for the acoustic energy to be reflected or refracted back to the surface is recorded, and the distance travelled by the high-frequency wave is computed from velocity data that were obtained previously from borings in the area of exploration. At depths of 900 to 1000 feet an interval of perhaps 30 feet containing rock of sharply contrasting velocities, as between firm sandstone or granite on the one hand and soft shale on the other, can be measured. The technique requires precise work with closely spaced "shot holes," using the best equipment. In this manner the position of the top and base of the clay layer might be determined at many closely spaced locations. If the basal contact is gradational, at least a minimum figure for thickness can be obtained.

In order to determine the suitability of the technique for application to the problem at SRP, it is necessary to have at least one complete sonic (acoustic) log of an uncased borehole, from the ground surface to a depth that is well into unweathered basement rock, for the purposes of (1) learning the amount of velocity contrast between the clay layer and both the sandstone above it and the crystalline rocks below, and (2) determining the velocity of the entire rock column through which the seismic energy will travel from the surface to the basement and return. Obviously several complete sonic logs would be desirable in order to obtain average velocities and to learn what variables might be anticipated. Unfortunately the only sonic logs now available are those that were run below the casing in the intervals of basement rock in six of the deep observation wells.

Shot holes and seismometer holes drilled with a small rig might be expected to cost about $1.00 per foot. An area of perhaps four square miles probably could be mapped at closely spaced intervals in about two months by an experienced crew at a cost in the neighborhood of $25,000 per month.

F. Deep-well disposal

1. *General considerations.*—Although disposal of liquid waste by injection into deep permeable formations has been considered for several years, important aspects of the subject have not been clarified. Rather, essential principles have been submerged in details which have slight significance to the over-all picture.

The term "deep" refers to any reservoirs in rock, not soil (see Glossary), which are below and so isolated from fresh-water aquifers that injected radioactive liquids will not obtain access to the aquifers either by natural means or by any process that is induced by the injection. In this context, we think of permeable sedimentary rock layers several thousand feet deep (generally at least 3000 feet) in geologic basins, confined above by thick relatively impermeable strata such as shale or salt deposits. Ideally, reservoir space that is suitable for deep-well disposal should be vertically restricted and laterally unrestricted.

Insofar as they could be determined with available data, the subsurface geologic conditions at all of the plant sites visited by the committee fail to meet the specifications stated in the preceding paragraph. At the same time, the Committee has never entertained the thought of using deep permeable formations as reservoirs for off-site disposals. Questions of transportation costs and safety engineering, therefore, do not enter into the Committee's considerations of deep-well disposals for radioactive wastes. The method is assumed to be a candidate for application to disposal problems only at suitably qualified sites, and in this light it is hoped that its advantages will be weighed carefully and applied as criteria in the selection of new plant sites.

All pore space in a permeable subsurface rock such as a layer of sandstone is occupied by naturally occurring fluids, principally water (commonly brine) but in some places petroleum oil or gas or nonhydrocarbon gas. The fluid may be

static or may be flowing, but in a tectonically stable basin at depths of several thousand feet, where we are dealing with *indurated rock* instead of loose soil or unconsolidated sand, the rate of flow is rarely more than about three feet per year. In 1000 years, therefore, under natural conditions a fluid *would not move* as far as one mile. In most instances the geologic basins that would be considered for deep-well disposal of radioactive wastes have *dimensions ranging from* tens to hundreds of miles in width. The natural flow is, therefore, insignificant and does not enter into computations in the following discussion.

2. *Present status of research.*—At present the subject of primary concern when considering deep disposals is the eventual fate of the introduced radionuclides—where will *they go, and how fast?* The answers to these questions are being sought in some of the research programs that have been supported by AEC. One such study is an early one at ORNL, *in which a slab of carefully* selected Berea sandstone, having as nearly as possible uniform porosity and permeability was treated as a laboratory-scale injection reservoir. A *five-spot system of injection* and relief wells was used to study flow patterns and rates and the dispersion of radionuclides. The movements of radioactive tracers were *monitored at grid points* in the 6′ x 6′ x 6′′ slab. In addition, the effects of ion exchange were studied in a cylindrical core of the sandstone 10 inches long and 1.75 inches in diameter.

Similar experiments, but on a larger scale in the field, have been conducted at the University of California and were planned at Bartlesville, Oklahoma, as described above under the heading "Historical Review."

Following the Bartlesville investigation, an attempt is now being made to find a more suitable *site for such field tests.* For economy and ease in acquisition, a site on federally owned or controlled land is preferred, and the following basic geologic requirements have been *specified for the experiments:* A flat-lying homogeneous clean sandstone, confined between impermeable strata in a structurally uncomplicated area; thickness between 5 *and 50 feet*; *depth between* 250 and 1500 feet; porosity between 10 and 30 percent; permeability between 160 and 1500 millidarcies. The objectives of experiments here will be to study under field conditions the possibility of determining the velocity of the radionuclides relative to that of water, to study the temperature effects, and to learn the extent of possible plugging of interstices by precipitates or by the swelling of clay particles resulting from any incompatibility between injected fluids and the natural fluids or minerals in the injection reservoir rock.

3. *Hydrologic considerations.*—A deep permeable reservoir contains brines having, in most cases, *greater salt concentration than sea water.* The composition indicates that fresh water has had only limited or no access to these reservoirs even during geologic periods of time. Nevertheless, *the geologic configuration* of any reservoir and the pressure pattern (pressure gradients or, more correctly, potential gradients) of the contained fluids should be well understood before it can be used to dispose of radioactive fluids. Furthermore, there should not be a chance that the brine will be produced either for its mineral content or *as a* byproduct of any associated petroleum. In addition, the disposal process itself should not cause the fluids to gain access to potable water reserves, either behind or through the casing in the disposal well or through fractures in overlying impermeable formations.

The *mechanics of disposing* of radioactive fluids in reservoirs differ from those for the disposal of any other fluid only in the precautions necessary to prevent escape of radionuclides *from the system, and thus it is* not only feasible but also highly desirable to use information that is available from the experience of various industries in the deep subsurface disposal of salt water and other liquid wastes.

The pressure required to inject fluids into a permeable formation creates the necessary space capacity in three ways: (1) it compresses slightly the native formation fluid, (2) by exerting stress on the surfaces of pore walls it causes the rock formation to expand slightly, and (3) more importantly, it artificially stimulates movement of the formation fluids. As such a flo wis radially outward, the highest pressures and pressure gradients are around the point of injection. When injection is stopped, the pressure increase disappears because a slight outward movement is sufficient *to accommodate* the expansion of the fluids and the contraction of the rock to their original condition. The relation between injection rates, pressure increase and the various *parameters of the formation* and fluids is well understood and will be discussed later in this chapter. The major source of difficulty lies in the collection and proper interpretation of *geological and* geo-

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graphical data which will define the characteristics of the permeable reservoir that is being considered for injection.

The disposal of fluids in any subsurface reservoir stratum is accomplished only by moving both those fluids and the displaced connate fluids through finely porous rock, an operation which demands the *application of energy, sometimes in* large amounts. The required pressure is always in excess of the natural pressure of the formation fluids, the realization of which leads to a rather widely held *belief that injection of large quantities* of liquid waste would require pressures in the well bore of such magnitude that not only the reservoir rock but also the confining impermeable *formations* would be fractured. Experience in the petroleum industry has deepened the understanding of the flow of slightly compressible fluids in *porous formations* and has shown ways to minimize the pressure increases that are needed for the injection of liquids at high rates. Because of their importance in evaluating the concept of deep-well disposals, we shall discuss these pressure relations in some detail.

It is well known *from theoretical* considerations confirmed by practical observations, that in disposal systems the increase in pressure is directly proportional to the injection rate and to the *viscosity of the liquid*, and inversely proportional to the thickness of the formation and to its permeability. Taking these relations into account in addition to the fact that the fluid *is slightly* compressible, the pressure effects of an injection rate of 500,000 gallons per day are illustrated in the following hypothetical example. The tables show the *increase in* pressure at the sandface and at various distances from the well at various times after injection was started. The results have been computed using the *formation* and liquid characteristics listed below and are collected in Table I.

Sandstone formation thickness=50 feet.
Permeability=50 millidarcies.
Porosity=30%.
Undisturbed reservoir or aquifer pressure=2500 psi.
Depth to injection reservoir rock=5000 feet.
Viscosity of injected fluid =0.5 centipoise.
Diameter of well bore=6 inches.
Compressibility of rock and fluid (relative to volume of fluid) $=7\times10^{-6}$ vol/vol/lb/inch.[3]
Transmissibility=1025 gal/day/ft.
Coefficient of storage$=45\times10^{-6}$ (dimensionless).

TABLE I.—PRESSURE INCREASE IN P.S.I. AFTER DAYS INDICATED AND AT VARIOUS DISTANCES FROM WELL

	1 day	10 days	100 days	2.74 years, 1,000 days	27.4 years, 10,000 days	274 years, 100,000 days
Distance (miles):						
1	0.5	19.0	55.4	94.6	134.0	173.8
2	0	4.3	32.6	71.0	110.4	149.9
3	0	.8	20.5	57.1	96.4	136.0
5	0	0	8.2	40.1	79.0	118.4
10	0	0	.5	19.0	55.4	94.6
15	0	0	0	9.2	41.8	80.7
20	0	0	0	4.3	32.6	71.0
30	0	0	0	.8	20.5	57.1
40	0	0	0	.1	13.0	47.4
50	0	0	0	0	8.2	40.1
60	0	0	0	0	5.0	34.2
75	0	0	0	0	2.3	27.3
100	0	0	0	0	.5	19.0
125	0	0	0	0	.1	13.3
150	0	0	0	0	0	9.2
Pressure increase in well, at sandface	318.5	358.1	397.7	437.4	477.0	516.6
Total volume injected (MM/gal.)	0.5	5	50	500	5,000	50,000
Area flooded (acres) displacement efficiency 100 percent	0.1023	1.023	10.23	102.3	1,023	10,230
50 percent	0.2046	2.046	20.46	204.6	2,046	20,460

The table shows that the major portion of the increase in pressure on the sandface occurs during the *first day after injection* was started, and that this pressure increases slightly less than 40 pounds for each ten-fold multiple of time. The table further shows the pressure increases that can be expected to occur in the

disposal formation. It will take 274 years (100,000 days) and an injection of 50 billion gallons of liquid before the pressure at the sandface increases from 2500 to 3017 pounds. It is clear from this behavior that it will take an additional 2740 minus 274, or 2466 years for the pressure on the sandface to increase another 40 pounds. Also, after 274 years the pressure in the formation one mile away from the disposal well will have increased by 173.8 pounds, about ⅓ of the increase that is observed at the well itself.

Very generally, the natural subsurface formation pressures, measured in pounds per square inch, are 0.45 to 0.48 times the depth in feet. Where, in the example cited in Table I, we mentioned a pressure of 516.6 pounds at the sandface after 100,000 days of injection operations, it should be kept in mind that this pressure is in excess of the natural undisturbed pressure in the reservoir of 2500 pounds and that only this amount of pressure increase is available for fracturing the rock. Once injection ceases, the pressure increase disappears almost as fast as it had begun, for the simple reason that from that moment the only movement of fluids in the formation is that resulting from expansion of the fluids and contraction of the reservoir void space. Because the expansion coefficient of both the fluid and the void space are small, only a little movement is required to reduce the increased pressures.

Table II shows the decay of the pressure increase in the well at the sandface, if injection is stopped after the number of days indicated. Within 24 hours after injection is stopped the pressure at the sandface is reduced by more than 300 p.s.i., regardless of how long the injection has been going on.

The thickness, porosity and permeability used in the cited example may be higher or lower than found in the selected disposal reservoir. The effect of lesser porosity is to increase inversely the area to be flooded by the stated volume of fluid. Lesser permeability would increase the pressure that would be required to inject the fluid at the specified rates. Inasmuch as each characteristic can have a value that is different from the one used in the example, the number of combinations is great but their effects can be computed. The values found at each site will have to be considered when appraising the merits of the reservoir for disposal of fluids.

In oil-field operations, the objective of artificial fracturing of oil or gas reservoirs is to improve the permeability of the rock in order to increase the flow of oil or gas to the well opening. At the same time it is desired to avoid the loss of oil or gas through leaks that might unintentionally be created in the overlying or underlying impermeable formations. No reports of such leakages have been found in the technical literature of the oil industry.

TABLE II.—EFFECT OF DISCONTINUATION OF INJECTION ON SANDFACE PRESSURES (p.s.i.)—(INJECTION RATE 500 GALS. PER DAY. SAND CHARACTERISTICS AS IN TABLE I)

	Injection continued for (and then is discontinued)—					
	1 day	10 days	100 days	1,000 days	10,000 days	100,000 days
Pressure increase in well at sandface at moment injection stopped	318.5	358.1	397.7	437.4	477.0	516.6
Pressure increase in well at sandface (p.s.i.):						
1 day after injection stopped	11.9	41.2	79.4	118.8	158.4	198.0
2 days after injection stopped	7.0	30.8	67.6	106.9	146.5	186.1
4 days after injection stopped	3.1	21.5	56.0	95.0	134.6	174.2
10 days after injection stopped	1.6	11.9	41.2	79.4	118.8	158.4
20 days after injection stopped	.8	7.0	30.8	67.6	106.9	146.5
40 days after injection stopped	.4	3.1	21.5	56.0	95.0	134.6
100 days after injection stopped	.2	1.6	11.9	41.2	79.4	118.8

Artificial fracturing is accomplished by the application of pressure through a column of fluid in the well, thus creating either vertical fractures, which usually occur at pressures that are about 0.7 to 0.9 times the weight of the rock overburden, or horizontal fractures, which are expected to occur at pressures in excess of overburden weight; exceptions to the rule have been observed. As the estimated weight of the overburden is commonly about one pound per foot or depth, the rock pressure in the example in Table I is about 5000 psi. Hence, the injection of fluids at a constant rate for 274 years into the described hypothetical reservoir would raise the pressure hardly enough to fracture the reservoir rock.

Upon discontinuance of the artificial pressures, the force exerted by the weight of the overburden will close the fractures, thus, in oil-field operations, defeating the purpose of the artificial fracturing. In order to avoid this result, coarse sand is injected along with the fracturing fluid, and the sand serves as a propping agent to hold the fractures open. Fracture treatments using from 25,000 to 50,000 pounds of sand are considered to be normal. The vertical extent of fractures can be observed by mixing the sand with short-lived radioactive material and logging its position by means of down-hole sensing devices. Experience in oil fields is that most of the fractures die out a short distance above the permeable reservoir.

In the case of deep-well disposals of radioactive wastes, it is clear that even if some fracturing occurred during injection operations, such fractures, unless artificially propped open, would seal themselves when injections are discontinued. Pressure increases which become excessive can be brought back to normal by interrupting the injection procedures as long as required.

Precautions such as the monitoring of bottom-hole pressures during injection, and surveying the hole with fracture-detecting instruments after test injections of nonradioactive fluids, can assure control of the operations, even where it may be desirable to fracture the reservoir rock purposely in order to improve injection rates.

As for the danger of fracturing the confining impermeable formations, it has been demonstrated that vertical fractures that are induced in petroleum reservoirs do not penetrate adjacent soft formations which have a high Poisson ratio relative to the reservoir rock. Fractures will propagate, however, through adjacent formations that are hard and brittle and have a low Poisson ratio, as exemplified by some dense limestones and quartzites. A requirement for a suitable disposal site, therefore, is a thick layer of soft rock such as shale or salt between the injection reservoir and any permeable aquifer or reservoir which is to be protected from invasion.

These discussions indicate that disposal by injection at high rates places a lower limit on the weight of the overburden and, hence, on the depth that is safe for containment. The weight of the overburden of a formation 1500 feet deep is about 1500 pounds; because the normal reservoir pressure would be about 700 to 750 pounds, only 300 to 400 pounds of additional injection pressure would lie within the margin of safety which would allow pressured injections without fracturing. Generally, the permeability of the formations decreases with increasing depth. The two desiderata oppose each other, and a compromise must be worked out on the basis of the known characteristics of all permeable formations which occur in the stratigraphic column at a prospective location. Proper site selection and preparatory studies can assure safe injection procedures where large volumes of liquid can be disposed of at relatively low pressures.

4. *General industrial experience.*—Several examples of subsurface liquid waste injection are cited in U.S. Bureau of Mines Circular 8212, "Subsurface disposal of industrial wastes in the United States," by E. C. Donaldson, 1964.* Cited injection rates and pressure behavior in consolidated and unconsolidated sedimentary rocks are generally similar to the data in Table I. Various chemical and mechanical treatments of the waste liquids facilitate trouble-free injection, and periodic treatments of the reservoir rocks are employed to remedy blockages caused by pore-plugging agents. The treatments are used routinely in oil-field brine disposals as well as in other industrial applications. Injected fluids should be chemically stable, bacteriologically sterile, and, where necessary, rendered nonreactive with respect to formational matrix and liquids. Inasmuch as the swelling of clay materials is fairly well understood, and techniques for its prevention have been developed, an examination of the permeable formations that are to be used for disposal will dictate the requirements that the injected fluid should meet in order not to cause plugging.

Reviewing the foregoing paragraphs, it can be stated that a large amount of injection technology exists that may be utilized in the deep-well disposal of radioactive waste liquids. Extreme care in the application of familiar techniques will, of course, be required. The particular reservoir conditions encountered, and engineering considerations that are unique to nuclear waste disposals, such as corrosion problems, safety measures, and programs for well reconditioning, will be the factors determining how this technology can be exploited. Engineer-

*Another recent publication of interest is U.S. Public Health Service Publication No. 999-WP-21, "Deep-well injection of liquid waste," by Don L. Warner, April 1965.

ing procedures for the necessary safety measures must be developed under the guidance of engineers who are experienced in deep subsurface operations.

For counsel and advice concerning questions of mechanical engineering in the construction, maintenance and rehabilitation of disposal wells, the Committee recommends appeal to the Society of Petroleum Engineers (Executive Secretary Mr. J. B. Alford, 6300 North Central Expressway, Dallas, Texas) of the American Institute of Mining, Metallurgical and Petroleum Engineers. The Society's membership includes petroleum and mechanical engineers who are experienced in procedures and problems in oil-well cementing and in various formation treatments such as artificial fracturing and acidizing.

5. *Conclusions.*—The Committee believes that, given a satisfactory geologic environment in the deep subsurface and the careful development of engineering procedures that will prevent escape of radionuclides during transit from the surface to the disposal formation, it is safe and feasible to inject radioactive liquids into deep permeable rock formations. It must be clearly understood that our concept of deep-well disposal does not permit any disposals by injection into formations that directly underlit fresh-water aquifers, or anywhere near them stratigraphically. Nor does it allow injections under such conditions that lateral flow through the disposal reservoir would bring waste fluids into the zone of potable water in the same formation. Disposal operations should be conducted sufficiently far from the rim of the basin, under such hydrodynamic conditions and at such great depths that accidental fracturing of an overlying formation would still not endanger a fresh-water aquifer. Where thousands of feet of strata including soft shale or salt overlie the disposal reservoir, no contamination of the biosphere could occur.

Considering the prospect that disposals of general industrial liquid wastes into deep subsurface formations will become more and more common in future decades, we foresee the likelihood of mutual interference by expanding circles of pressure influence from several disposal operations in the same reservoir formation. If three or four operations are conducted simultaneously in one disposal reservoir, even though the points of injection should be 50 or 100 miles apart, it can be seen from Table I that in a period of a few decades the areas of pressure increase will meet. Eventually the pressure increases will travel back to the injection wells, and the rates of increase will gradually accelerate. Certain limitations might then have to be imposed on further injections. These will not be great immediately but will increase as injections continue. The prospect requires that complete and accurate records be kept of pressure data in the injection wells and in observation wells that will need to be drilled for that purpose. Legal restrictions of multiple usages of reservoirs having prior demand may some day be necessary.

In the same line of thought, competition for available reservoir space also would dictate that any waste liquids should be concentrated as much as possible before injection. In the case of radioactive wastes, this would mean the injection of intermediate rather than low-level wastes, in which the limit of concentration would presumably be dictated by considerations of heat dissipation in the subsurface.

We return now to the subject of the proposed field experiments, such as the one which was attempted at Bartlesville, which are designed partly to learn about the relative velocities of water and radionuclides in an injection operation, and we reiterate our position that no deep-well disposals should be undertaken under conditions where injected liquids might under any circumstances reach the biosphere. In view of the logical presumption that radionuclides will travel no faster than the liquid which contains them, we see little need for the further expenditure of funds to study the relative velocities of radionuclides and water as any part of a deep-well disposal program. The value of such studies is limited in application to procedures in which it is anticipated that the waste liquids will remain in the biosphere or will return to it before the nuclides have decayed to a harmless level, as in disposals in soil. Insofar as studies of heat dissipation and of pore-plugging are concerned, this information is pertinent to deep-well disposal, but because of local variations in lithology from place to place, the most useful results can be obtained by pilot operations in the selected deep disposal reservoir at an actual plant site.

G. Geophysical studies at NRTS

1. *Geological background.*—**The National Reactor Testing Station, in southeastern Idaho, lies on the Snake River Plain, which is both a topographic basin**

and a structural depression. Paleozoic rocks crop out in mountains to the north and are overlain by upper Tertiary (Pliocene) volcanics, apparently without any intervening strata. Lying on the Tertiary volcanics is the Snake River group of basalt flows and interflow sedimentary deposits. The surface is a veneer of alluvial, lacustrine, and aeolian deposits, except where basalt and other volcanic rocks are exposed.

The character of the basalt ranges from dense to vesicular to blocky. Some of the basalt flows are excellent aquifers, others are essentially impermeable. The total thickness of the basalt series exceeds 1500 feet, this figure being the maximum depth that has been reached to date in borings. The associated sedimentary deposits, similar to those of the surface veneer, are gravel, sand, silt, and clay, the porous and permeable beds among them also being aquifers.

The surface of the plain ranges in elevation between 4800 and 5000 feet above sea level. The water table descends from an elevation of 4600 feet at the northeast to 4400 feet at the southwest. The unsaturated interval, or zone of aeration, is therefore 200 feet to at least 600 feet thick. This is the interval of so-called "dry soil" above the water table which NRTS utilizes as a reservoir for the disposal of radioactive waste liquids.

Neither the depth to relatively impermeable basement rocks nor the depth to the base of the fresh-water zone, the collective group of aquifers, is known; only fresh water occurs in the deepest hole that has been drilled to date. The continuous need for more detailed and more accurate data regarding the distribution, capacity and interrelation of aquifers in the Snake River Plain led to a program of geophysical surveys, both by borehole logging and by ground and aerial mapping.

Data obtained from borehole logs are to some extent supplementary to information derived from laboratory analyses of samples, but when such data are properly interpreted they also provide additional information which is not otherwise obtainable. The methods of geophysical mapping provide interpretations of the regional configuration of various subsurface layers of rock. In areas of complex strata, such as the NRTS, the interpretations are imprecise at best, and in the absence of supporting data obtained directly by drilling they are highly speculative.

Thorough evaluations of geophysical operations cannot be made in brief examinations such as those afforded to the Committee during its rapid tour of the sites last spring. This statement is particularly true of geophysical mapping, in which much depends on the amount and quality of basic data and on the manner of their day-to-day applications. Minor questions of geophysical-log interpretations were discussed at the sites, and the Committee's overall reaction to the work is favorable. The staffs appear to be competent and well informed. Following is a brief summary of the geophysical methods that were discussed, together with an annotated list of other techniques which might some day be useful.

2. *Borehole logging.*—Techniques included under this heading involve the lowering of mechanical, electrical, electronic, radiation-sensing, or other instruments into a borehole for the purpose of making a continuous record or log of the physical properties of the rocks or fluids throughout the full depth of the hole.

(*a*) *Electric log.*—The instrument records two basic parameters in boreholes filled with fresh (nonsaline) drilling muds: the spontaneous potential (s.p.) and the resistivity of the rocks to the flow of electric current. It cannot be used in a cased hole or in one which contains no water. In oilfield operations it is widely used and relied on for correlations of rock strata from hole to hole, and for estimates of qualities such as porosity, permeability, and fluid content in the rocks. As pointed out by operating personnel at NRTS, the electric log is of only minor value in southeast Idaho because both fresh water and dense basalt are highly resistive, and the resulting log lacks character.

(*b*) *Gamma ray log.*—This instrument was designed to detect and measure the amount of natural gamma radiation in the rocks that were penetrated by the drill. It can be run successfully in either cased or open holes, either wet or dry. Its usefulness is found primarily in the distinction and identification of different gross lithologies and, therefore, for stratigraphic correlations from hole to hole. At NRTS it is useful in the identification of such lithologies as basalt, tuff, and shale. It is important for operators to bear in mind that such interpretations need confirmation by the examination of drill cuttings in selected

boreholes, not too widely spaced, in order to avoid misinterpretations as lithologies varies from place to place.

A more obvious use of the gamma ray log in radioactive-waste disposal operations is in the detection of introduced gamma radioactivity in the subsurface and, therefore, in mapping the extent of contamination in aquifers.

(c) *Density log.*—The density logger is a device in which a gamma ray source and a shielded detector are held against the borehole wall. The tool measures the induced gamma ray intensity in the wall rock, a function of the formation density; porosity may be estimated from these data. Many corrections must be applied to the log readings, because the instrument is highly sensitive to mud-cake, and to borehole roughness (rugosity), and gas content of formation liquids. Density logs are more dependable in high-porosity formations(greater than 10%) than in low-porosity rocks. The effects are based on diffusion processes, and assumptions are made that all interactions measured take place in the formation and not the borehole. Further, natural gamma radiation is not counted or is negligible.

A new "compensated-density log" has been introduced by Schlumberger Well Surveying Corporation which is said to correct automatically for hole size and mud-cake thickness. Used in combination with other logs, the density log can give a fairly reliable indication of rock porosity.

(d) *Caliper log.*—As the drill penetrates various layers of hard and soft rock, compacted or loose, fractured or whole, the diameter of the hole varies because of varying bit action or caving of the walls. The caliper log is made by a mechanical device which measures continuously the diameter of the hole. Used partly in conjunction with the density log, but also advantageously with other logs, the caliper log gives useful information about rock characteristics as well as about the condition of the hole.

(e) *Flow meter.*—This instrument incorporates a spinner device which, emplaced in a fixed position in a borehole, indicates the upward or downward direction of flow within the hole, and measures the rate of flow. It has been used commonly at NRTS and has provided useful data regarding vertical components of flow within an aquifer and between aquifers.

Another type of flow meter, the Tracejector, has been tested successfully at NRTS. The velocity and direction of flow between two points in a well are measured by injecting a radioactive tracer at one point and recording its arrival time at a second point.

(f) *Temperature log.*—The logging of water temperatures in a well indicates the presence and depth of water which is warmer or cooler than the natural ground water. Such abnormal temperatures reveal the presence of "foreign" waters introduced into the normal ground-water circulation. The log also provides temperature data for use in connection with water-resistivity logs.

(g) *Water-resistivity log.*—Natural ground water at NRTS is very low in dissolved solids, averaging about 250 ppm, whereas introduced waste liquids are slightly more saline, with salt content in the neighborhood of 1000 ppm. Because of this chemical difference, the difference in electric resistance is a direct guide to the occurrence of "foreign" water in the aquifer. The water-resistivity log provides a convenient record of deviations from normal resistivities.

(h) *Specific-conductance log.*—In contrast to the foregoing records, this log is produced not by downhole logging but by combining the data from temperature and water-resistivity logs. The product gives a more accurate idea of water composition than does either of the separate logs, and so provides a more reliable identification of the water.

The study of specific-conductance logs and flow-meter logs together forms the basis for the interpretation of flow in boreholes. This practice of combining several types of logs, including the lithologic and petrographic logs prepared by the geological staff, is to be commended and encouraged. The interpretation of poorly defined characteristics in logs can thus be enhanced, and obsecure properties or occurrences often can be revealed or magnified.

The usage and interpretation of logs at NRTS conform to practice in oil fields, where the technique of downhole logging was developed. Our only qualifying comment is to caution against over-reliance on such logs without constant support from laboratory analyses of samples of the fluids and rocks whose properties are presumably being measured by the logs.

Many other logs are available for borehole investigations. Some of these may be useful in future geophysical studies in the Division of Reactor Development

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and Technology research program. Of particular interest are the suites of neutron logs and acoustic logs which are described briefly in the following paragraphs.

(i) *Neutron logs.*—Neutron-logging devices all measure radiation characteristics of substances irradiated from a neutron source lowered into a borehole:

(1) *Neutron-epithermal neutron log.*—Epithermal neutron distribution is measured as a function of hydrogen (water or oil) in the formation. The main advantage is that the tool can be run at reasonable logging speeds if a strong source and efficient counter are used. Disadvantages include the sensitivity of measurements to influences such as borehole fluid, cement condition, degree of mud filtrate, and position of the tool in the hole.

(2) *Neutron-thermal neutron log.*—Thermal neutrons are counted as a function of the amount of hydrogen and chlorine in the formation. The system has the same advantages as the neutron-epithermal neutron log, but salinity and porosity cannot be separated. Disadvantages of both systems are similar.

(3) *Neutron-gamma log.*—Gamma rays produced by capture of thermal neutrons are detected by the borehole counter. The source of these gamma rays is correlative with thermal-neutron density. If porosity is known, salinity can be inferred from the flux of high-energy gamma rays which chlorine, in particular, emits. Proper tool design permits good sensitivity for detecting chlorine through casing. A disadvantage lies in the lack of good agreement between theory and experimental data.

(4) *Pulsed-neutron log.*—Neutrons from a pulsed tool source interact with the formation, and the resulting thermal-neutron flux decay is determined. The slope of the decay is related to the presence of chlorine in the formation fluid. In terms of sensitivity, near-borehole effects, and instrumentation, pulsed-neutron logging is far superior to the steady-state methods. The tools are not widely available yet, and future developments may see this tool's greatest use in porosity determinations and in differentiating between oil, fresh water, and brine. The major problem at present with this tool is the presence in rock of absorbing elements such as boron, lithium, and the rare earths. These elements are believed to account for the much shorter neutron lifetimes in boreholes as compared to theory and laboratory model experiments. When these problems are resolved, the pulsed-neutron log will be the most effective "through casing" log available.

A modified pulsed-neutron tool measures decay of intensity of gamma-ray flux produced by neutron capture. The gamma-ray counting rate is related to the reciprocal of neutron lifetime in the medium being investigated. Thus, the log is a "lifetime log." The measurements are chiefly a function of chlorine in formation fluids, as with the other logs. However, near-borehole effects are small, and asymptotic decay of gamma rays occur sooner than does decay of neutron flux. This tool is still in the developmental stage.

(j) *Acoustic (sonic) logs.*—These tools consist of a sound source and two or more receivers spaced at known distances from the transmitter. Travel time of acoustic energy (compressional wave, shear wave, tube wave) between receivers is measured. Given the travel time and assuming that the propagation path is known, acoustic velocity of the rock formation is inferred:

(1) *Acoustic-velocity log* (compressional wave).—Travel time of an acoustic compressional wave is measured and velocity is calculated. Velocity logs are used to compute reflection coefficients and vertical velocity "profiles" for seismic investigation. The logs can also be used for correlation purposes. By far the greatest use of the velocity log is in estimating the porosity of subsurface rocks, once the effective stress, matrix material, and average pore size are known. The log is also used for velocity determinations in connection with seismograph surveys.

(2) *Acoustic-velocity log* (shear wave).—Shear-wave travel time is measured between receivers. In certain formations (limestone, anhydride, salt, dolomite, and well-cemented sandstone) shear-wave amplitude is larger than the compressional. Thus, with independent determination of compressional and shear-wave velocity plus density, Poisson's ratio for the rock is obtained, and all of the remaining elastic constants may be calculated. These approximations have proven to be very satisfactory.

(3) *Acoustic-amplitude log.*—All three waves (compressional, shear, and tube) are measured. Attenuation of low-frequency components of tube waves is dependent on formation rigidity, which in uniform lithology changes as a

result of fracturing or fluid saturation. Commercial application of this type of log has been in attempting to detect fractures in open holes. The tool is sensitive to borehole centering and caliper, mud properties, hole size, and bed boundaries. Small shale stringers are not distinguished from horizontal fractures. Modifications of this logging method are used to determine the effectiveness of cement bonding between casing and the hole wall.

3. *Geophysical mapping.*—A number of geophysical mapping techniques have been developed, primarily in petroleum exploration, for the purpose of learning about strata which have not yet been reached by the drill. All of them record physical properties of rocks which vary as the lithology varies. They are more rapid and less costly than exploration by the drill, but the data derived from such surveys can rarely be interpreted precisely or uniquely in terms of rock distribution and properties. Resulting maps, therefore, are less reliable than those constructed from data that could be derived directly from closely spaced boreholes.

Geophysical mapping is commonly the first step in the exploration of an area such as the NRTS where deep-borehole data are lacking. Under favorable circumstances the gross form and depth of a basin may be delimited by gravity and magnetic work, as attempted here. Indications of more localized rock distribution and structures may be obtained with suitably designed programs.

The geology at NRTS presents the geophysicists with particularly difficult problems. The rocks filling the Snake River depression are a complex of basalt lava flows which are massive to scoriaceous and cindery and have interbedded lenses and tongues of alluvial and lacustrine clays, sands and gravels. The sequence is thus extremely heterogeneous, both laterally and vertically, with regard to porosity, permeability, density, magnetic properties, seismic velocity—in fact all the physical properties which form the basis for geophysical exploration. The lavas and interbedded sediments were laid down in an easterly trending trough, the Snake River downwarp, on older rocks whose structures trend more or less north-south. These older rocks were folded, faulted and deeply eroded before the Snake River downwarp developed. Presumably the older structures pass beneath the downwarp approximately at right angles to its strike.

A principal object of the geophysical program, namely the determination of the depth and configuration of the basement surface on which the lava sequence was poured out, is thus complicated by the basement structural pattern. Both gravity and magnetic anomalies may reflect topographic relief on the ancient surface, or masses of rock of contrasting lithologies, or varying rock patterns in a structurally disturbed area. The variables are such that no unique solution is possible without deep-well data on physical properties of the rocks and depth to basement.

Hence the value of the existing geophysical-mapping program would be greatly enhanced by NRTS's proposed deep drill hole, especially if this hole extends into basement beneath the basalt sequence. Data from such a well would permit the definition of the basement surface for a considerable area surrounding the well. The degree of confidence in the reconstruction would, of course, diminish with distance from the well. A reasonably accurate delineation of the basement surface and its hydraulic characteristics over the entire NRTS, presumably would require a minimum of three deep wells penetrating the entire basalt sequence and going into the basement rocks far enough to give an indication of their porosity, permeability and fluid content.

(*a*) *Gravity and aeromagnetic surveys.*—The operation of a gravimeter is based on differences in density between different types of rocks. A mapped positive anomaly could mean (1) the presence of a relatively small mass of dense rock at a shallow depth, surrounded by less dense rock, (2) a large discrete mass of dense rock at great depth, or (3) an arched uplift which brings a deep layer of uniformly dense rock closer to the surface at the location of the anomaly. The accuracy of the interpretation depends on (1) the nature and complexity of the geology, (2) the operator's experience, especially in the type of subsurface geology that is being explored, (3) the spacing between gravimeter stations where data are obtained, and (4) the amount of subsurface data available from bore-holes.

The operation of a magnetometer is closely analogous but is based on measurements of magnetic variations rather than densities. The problems in interpretation are similar. The magnetometer has the advantage, however, that readings can be made instantaneously and from a moving instrument. Consequently,

techniques have been developed for making magnetometer surveys by trailing an instrument behind an aircraft. The amount of detail that can be obtained depends upon the flight altitude and the distances between traverses. In reconnaissance work excessive detail is apt to be more confusing than helpful.

Gravity and aeromagnetic surveys were conducted for AEC at NRTS in 1964, and the resulting data were added to those available from earlier surveys. Interpretations of the gravity data are complicated by the fact that the density of basalt varies widely from flow to flow and from place to place within a flow. Although basalt is generally strongly magnetic, it varies also in degree of magnetism and in direction of polarization. The final products of both surveys are contour maps. Interpretations of observed anomalies are speculative. Some faulting is suggested by the contours, but the evidence is unreliable without better control.

The densities and magnetic properties of rock specimens from adjacent areas have been determined by laboratory measurements, but deep subsurface control from the NRTS is desirable to improve the interpretations.

(*b*) *Seismograph surveys.*—Seismic surveys involve the transmission of acoustic energy, usually from a near-surface explosion, downward into subsurface rocks. This acoustic or "seismic" energy is in part transmitted through the rocks and in part reflected and refracted back to the surface where the arrival times and energy characteristics may be measured. Seismic-reflection surveys are most commonly used by the petroleum industry to determine the depths and structural configuration of subsurface rocks. For technical reasons this methods does not normally give good results at depths less than about 500 feet. The reflection menthod is not well suited to exploration of thick masses of basalt. It is especially dobtful that satisfactory results would be obtained from the highly lenticular basalts and sediments of NRTS, except possibly for study of selected local structures.

Refraction surveys can be used effectively at shallow depths to determine the thickness of sediments above the first basalt flow. It is not very likely that useful data will be obtained by this method at greater depths in this geologic setting, because the irregular alternation of high-speed and low-speed zones within the lava would give a confused seismic response.

(*c*) *Aeroradioactivity survey.*—This work involves the recording of natural radiation from the earth's crust by means of an airborne instrument. The effects of any artificially introduced radioactivity would, of course, also be sensed. Because only preliminary work has been reported, the Committee is unable to comment on the results.

4. *Conclusions.*—Throughout all of the geophysical programs at NRTS the need for petrophysical data from boreholes is obvious. The value of the interpretations is directly proportional to the amount and quality of supporting analytical data. This relation is made clear in projects where such data have been obtained. The geophysical program is still active and should be continued until the usefulness of the methods has been exhausted. Particularly, we urge the need for more geophysical-log studies of the interaquifer flow of ground waters, because the overall flow system is still inadequately understood.

H. Ground disposal of gaseous wastes

AEC is considering the vexatious question of handling radioactive gases that might leak from a reactor's containment shell in the event of melt-down resulting from a rupture in the internal cooling system. Among other procedures it has been suggested that the vapors released at high temperatures into the shell be pumped into a ground-water aquifer. Inasmuch as the injection would have to be completed quickly, it was further suggested that the subsurface reservoir be prepared in advance by the injection of air so as to displace an appropriate amount of water, thereby producing a huge air bubble in the aquifer.

The Committee doubts that this procedure would be practical, because over a period of time the air bubble would move along the aquifer, eventually becoming dispersed, and therefore a stream of air would have to be injected from time to time throughout the life of the plant in order to be ready for an emergency. Under unusual circumstances in which a domal structure or a lenticular formation makes a local trap, the procedure might be effective.

It has also been suggested that the zone of aeration, the unsaturated interval lying above the water table, be utilized as a receptacle for radioactive gases. Few plant sites, however, have an aeration zone thick enough to provide a useful

capacity for such injections. Moreover, such a disposal procedure for gases would be subject to the same objections as for liquid wastes—i.e., the danger of possible migration into and along the aquifer, particularly under the influence of moving waters.

VII. OPERATIONAL PROCEDURES

In addition to the Research and Development projects described in preceding pages, the Committee has considered also some broad geologic and hydrologic aspects of other ground disposals. They pertain to practices which are in general use at the sites visited by the Committee and which are part of the whole range of disposal procedures in which geologic aspects bear heavily on the long-term safety of the operations. Attention is called to the outline of the geology of the four major plant sites where disposal techniques are being developed (pp. 14–18 of this report). Present operational procedures are considered in three categories, as determined by the levels of radioactivity in the wastes.

A. High-level wastes

Both liquid and solid high-level wastes are now being stored in underground containers built of steel and cement. The operators consider the tanks for high-level liquids as being only an interim device pending the development of safer techniques for permanent disposal. Procedures for solidification of the liquids are being developed, and it is anticipated that the tanks eventually will be used only for temporary storage during initial decay and cooling stages.

Methods for disposal of the products of solidification, like disposal of saturated or partially saturated adsorbants and of high-level radioactive trash (e.g., used laboratory equipment) remain to be perfected. If the solid product will be essentially insoluble for a period of 600 to 1000 years, and if sorbed radionuclides will not be released from contaminated solids during the same period, the solids can be stored safely at shallow depths in the ground above the water table. However, the permanence of fixation on trash or perhaps even on used ion-exchange materials cannot be relied on, and such a disposal site would still carry the hazard of accidental discovery and release to the biosphere in the event of excavation in future centuries when knowledge of the burial may have been forgotten. Storage below the fresh-water zones, particularly in salt beds, would appear to be the safer procedure for all manner of high-level solids.

B. Intermediate-level wastes

Treatment of intermediate-level wastes varies from site to site, as also does the definition of what constitutes intermediate levels. Solid trash is buried at shallow depths in earthen trenches at all sites; radiation levels in adjacent soil are monitored in order to avoid concentrations higher than accepted safe levels, but there remains the risk of unanticipated movements of the radionuclides as changes in the chemical or hydrologic environments upset the ion-exchange equilibria.

Treatments of intermediate-level liquids by evaporation in order to reduce their volume and by ion-exchange materials to reduce their radioactivity, have been investigated, and such systems are being used. At sites in semi-arid lands the intermediate-level liquids are pumped into seepage cribs and trenches, their subsequent movements through the earth being monitored through borings. The desired objective in controlling such disposals is to avoid appearances of radioactivity in excess of a small fraction (usually one-tenth) of MPC at the nearest point of use downstream.

C. Low-level wastes

Low-level trash is buried at shallow depths. The characteristics of low-level liquids, and disposal methods, vary from plant to plant. Very large volumes of cooling water, for example, which normally has little or no radioactivity but which may occasionally receive accidental discharges of radionuclides, are generally directed into ponds, swamps, or surface streams from which they eventually find their way into the major rivers. Assuming constant monitoring which would permit the operators to direct the flow temporarily into more confined reservoirs for detention in the event of serious contamination, this procedure appears to be harmless.

Other low-level liquids at some sites are pumped through disposal wells directly into fresh-water aquifers, the operators relying on long travel times and

on sorption phenomena in the aquifer to reduce the radioactivity to safe levels before the contaminated water reaches downstream points of use.

The low-level liquids contain all manner of radionuclides, most of which are short-lived. At all locations, however, some of the isotopes are long-lived, the more common ones being tritium, cesium[137], strontium[90], and cobalt[60]. Not all of these are equally hazardous to man, and the degree of hazard is not directly proportional to their longevity; strontium[90] and cesium[137] are the ones of greatest concern because of both their longevity and biological hazards.

The amounts of the long-lived radionuclides that are put into the ground with low-level wastes vary from year to year and from location to location, as do the total amounts of all radionuclides. Radioactivity produced by cesium[137] and by strontium[90] ranges from a small fraction of one percent in some places to more than 10 percent at others, for each of these isotopes. In some places tritium accounts for more than 99 percent of the released radioactivity at certain times.

At present the amounts of total activity produced by waste are kept well below RCG levels between the disposal areas and the nearest points of use which are beyond the reservation boundaries. In some cases these distances are great, and large areas of intervening land can become contaminated before measurable amounts of activity reach the producing wells or streams. Moreover, the first appearance could be the fringe of a dispersion band which would be followed later by the zone of concentration. Monitor wells are located between the disposal areas and the nearest points of use, most of them clustered around the disposal facilities, and it is at these points that the criterion of RCG levels should be applied in all cases as it is now in some.

D. Geologic factors in appraisal of operating procedures

In nearly all of the ground-disposal procedures which currently are part of routine operations, two factors are depended on for protection of the biologic environment: (1) the capacity of soil and rock to contain much of the radioactivity by sorption phenomena until decay to innocuous levels, and (2) long travel time in respect to decay time. Both factors are governed by parameters which can be and constantly are being determined empirically, both in the laboratory and in the field. At present, therefore, no serious hazards have been created by the current disposal operations.

The Committee, however, is concerned about the long-term safety of the operations if they are to be continued at the same sites for many decades or even for centuries. The ion-exchange equilibria are reversible, and under changing conditions desorption could replace adsorption. Such a changed situation might result from man's future activities which would affect the hydrologic systems in the aquifers as dams are built, irrigation is accelerated or extended into new areas, or ground-water aquifers are subjected to higher withdrawal rates, all of which are not only possible but likely happenings in forthcoming decades. Climatic changes in future centuries may also be sufficient to alter substantially the hydrology in an aquifer and thereby affect its sorption capacity.

The concept of "delay and decay" is applicable to many components of radioactive wastes, but the long-lived isotopes such as strontium[90] and cesium[137] are components whose threat is reduced hardly at all in a few decades. As the adsorbant earth materials under a disposal site become saturated after long-continued disposal of even low-level wastes, the concentration of radioactivity eventually could become equivalent to that which would result from lesser volumes of higher-level wastes, depending upon the rate of accumulation of long-lived nuclides. In such an event the disposal concept then becomes one of "concentrate and contain," but in the event of altered hydrologic conditions the prospect of containing the concentrated long-lived radioactivity becomes dim. The movement of radionuclides in the subsurface is bound to be extremely difficult to control in the absence of unsaturated natural absorbants.

With the above considerations in mind, the Committee is especially anxious about continued ground disposals of any level of wastes containing long-lived radionuclides unless they be emplaced below the fresh-water aquifers at such depths and under such hydrologic conditions that they will not be returned to the biosphere.

The Committee is also dubious about the concept that in arid and semi-arid lands meteoric water does not percolate downward as far as the water table but instead is lost entirely by evaporation and plant transpiration. Operations conducted under this concept are saturating tangible volumes of soils with radionuclides, some of them long-lived, which in the course of a century or two may

be carried to the water table by catastrophic "once-a-century" deluges. Especially in areas having centripetal surface-drainage systems and where innumerable local topographic depressions collect runoff, contamination at or near the surface may be subject eventually to transportation downward to the water table in the event of a rare calamitous flood. The Committee is not aware of any past or present research that might aid in determining the extent of this risk. It is a field of investigation which should not be overlooked as a part of the research pertinent to ground disposals of long-lived radioactivity.

VIII. DEEP EXPLORATORY DRILLING

A. National Reactor Testing Station

At present no one knows where the base of the fresh-water system lies in the Snake River Basin. Nor is anything known about the lithology of the rocks at those unknown depths. It is important to learn as much as possible about the geology and hydrology of the deep rocks in order to understand the complete hydrology of the NRTS site as a further guide to the fate of radioactivity that is released in it.

The drilling of a deep exploratory well at NRTS has been recommended by the staff there, and we strongly support the recommendation—including continuous coring, geophysical surveying by a number of logging devices, measurements of pressure or static heads, determination of the rates of buildup or drawdown, and collection of fluid samples at frequent intervals. In this well as in all exploratory borings, shallow and deep, the properties of the rocks with respect to movement of fluids, and the composition of fluids, should be determined by laboratory analyses of samples rather than solely by interpretation of geophysical logs. More than one deep exploratory well may eventually be needed in order to gain the essential three-dimensional view of the deep hydrologic regimen.

The objectives cited by NRTS are desirable and necessary. If such a deep test is scheduled, NRTS may find it helpful to obtain the services of one or more geological and engineering experts who have had long experience in exploratory drilling in similar terrain in the petroleum industry.

B. Hanford Atomic Products Operation

An oil company drilled a wildcat well in the Rattlesnake Hills in 1957–1958 to a total depth of 10,655 feet, where it was abandoned as a dry hole. The drill-site is near the southwest edge of the Hanford area. At the total depth the drill was still in Tertiary volcanic rocks, and the operators reported fresh water at the bottom; however, no analysis of the water was made. We support the view of Hanford personnel that the hole should be reentered for the purpose of sampling the fluids, obtaining pressure measurements, and running additional geophysical logs.

Nothing is known at present about the hydrologic system beneath the volcanics in the Pasco Basin, or even about the lithology. For that matter, there is little information of any kind about rock formations and their contents below a depth of about 2000 feet. The nature of the fresh-water aquifer systems in the deeper part of the volcanic series is critical to complete understanding of the eventual fate of all foreign fluids and nuclides that are accepted by the fresh-water aquifers. For these reasons we urge that efforts be made to reenter the Rattlesnake Hills boring and conduct necessary tests, as a prelude to any further deep drilling that may be required later.

IX. DISPOSAL VAULTS IN DESERT HILLS

It has been suggested that a tunnel or other excavation in the Rattlesnake Hills at Hanford might be a safe place for disposal of dry radioactive solid wastes. The hills rise more than 1500 feet above the surface of the Pasco Basin to the northeast, and even more above the water table. Precipitation on the hills appears to be light. A similar situation might be found at Middle Butte at NRTS. Such situations might provide suitable dry storage or disposal facilities for radioactive solids of many types, including baled trash and such objcts as those which are currently stored on railroad cars in tunnels under the plains. Proponents of the idea emphasize that the chief advantage of such a site would be its elevated position where runoff cannot collect. Care would need to be taken to protect the contents from drenching, leakage, and runoff or percolation of contaminants to the outside ground surface or downward to the water table.

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However, too little is known about geologic factors in these hills to enable one to recommend any construction operations. It is necessary to know more about details of local stratigraphy, lithology and structure, especially jointing. As a preliminary measure, the hills should be mapped in detail, in order to determine the position and attitude of the thickest and least-brecciated lava flows, because sound basalt would probably hold openings that are permanent and virtually indestructible. In order to determine the feasibility of the proposal, geological studies are needed along the lines suggested.

X. GENERAL CONCLUSIONS

A. Criteria

The Committee's criteria in evaluating disposal methods are: (1) permanent isolation from the biosphere of all hazardous concentrations of radionuclides; (2) protection from long-lived radioactivity must extend for 600 to 1000 years; (3) when safety is involved, cost is secondary.

B. Costs

Lack of funds has been cited at one location or another for inability to conduct needed research or to use alternate disposal methods which are agreed to be safer than current practices. The Committee recognizes that decisions on expenditures must be based on many factors, often including some that may not be known to all staff employees. However, it is apropos to point out that waste-disposal costs are now a small part of the overall expense budget of the nuclear industries, and that any compromise with safety for the sake of economy could lead, in the long run, to a mushrooming of waste disposal into the most costly item in the use of nuclear power.

C. Evaluations

1. *Progress in new techniques.*—The Committee is pleased with progress in methods for solidification of high-level liquid wastes, burial of high-level solid wastes in salt beds, and, as far as its present application at ORNL is concerned, the injection of grouted intermediate-level wastes into shale fractures. There are some reservations regarding the capacity of the subsurface at ORNL for continued injections of grout over periods of many decades, and this method therefore should be re-evaluated periodically.

2. *Disposals to the environment.*—The Committee thinks that the current practices of disposing of intermediate and low-level liquid wastes and all manner of solid wastes directly into the ground above or in the fresh-water zones, although momentarily safe, will lead in the long run to a serious fouling of man's environment. Such methods represent a concept of easy disposal that has had and will continue to have great appeal to operators, but we fear that continuation of the practices eventually will create hazards that will be extremely difficult and expensive to eliminate. Although the ion-exchange capabilities of natural earth materials under disposal sites will retain quantities of radionuclides and provide a safe container for the shorter-lived ones, it would appear to be prudent to reserve a large portion of the capacity for accidental releases—especially in humid regions where the water table is shallow and distances between disposal sites and discharge points are small.

At ORNL disposals into surface pits in a shale formation have been troublesome because of some seepages and overflows. The contaminated areas adjacent to the pits now offer a unique opportunity to study in a "field laboratory" the migration of nuclides through earth materials. Particularly the situation provides an opportunity to determine whether or not earlier assumptions regarding the permeability of brittle shales might have been erroneous, to map migration routes and rates, and to learn to what extent lithologic variations in the shale may affect calculations of ion-exchange capabilities.

At NRTS all operations are conducted over one of the largest of the country's remaining reserves of pure fresh water. In addition to present regrettable contaminations by salty wastes from irrigation and industrial effluents, contamination by radionuclides is a prospect which must be constantly guarded against. The same protection is needed for the large reserves under the Hanford site, where irrigation and dam projects are crowding upon the boundaries of the reservation. The effects of irrigation developments on the Wahluke Slope north of Hanford's "100" area, for example, should be a matter of concern and study. Another potential hazard at Hanford involves the possible flooding of sectors along the Columbia River in the event of a landslide from White Bluffs into the lake which is to be created by construction of the Ben Franklin Dam. These are the

kinds of events which in the course of future decades may substantially alter the hydrology of aquifers under the disposal sites.

These possibilities and the need to protect diminishing reserves of potable ground water strongly influence the Committee's appraisals of current and proposed waste-disposal practices. All proposals for surface or near-surface storage of calcined high-level waste products in semi-arid regions where the "dry soil" above the water table is assumed to be a safe container, should be examined carefully in the light of possible *events of future* centuries. Even storage of these products in surface tanks or bins as a permanent disposal appears to be risky.

3. Problems in geohydrology.—As part of the research and *monitoring* program, NRTS has been investigating vertical flow in wells, with the desirable objective of learning more about interflows from one basalt aquifer to another, lower or higher—a subject of importance about which little is now known. We suggest that the *effects of* the extreme anisotropy of the basalt aquifers also need more adequate consideration, both at NRTS and at Hanford. Rates and direction of ground-water movement, both horizontal and vertical, should be determined as precisely as possible. We hope that the *staffs at these sites* will continue and extend their studies of vertical flow by means of flow measurements, in uncased holes as much as possible, and also by means of static heads measured at different depths in the same hole. We think further that trace injection and modern methods *for determining* borehole water movement should be put to use whenever pumps are pulled from existing wells and in all newly drilled wells. The results of monitoring the movements will be misleading if waste fluids should bypass monitoring sites. *This* bypassing can happen easily in ground-water systems as deep as these, complicated by the peculiar geologic features of basalt and intercalated sedimentary beds and by the prevailing hydrologic characteristics of extremely high transmissibility and extremely low storage capacity.

Further in regard to the geohydrology at the semi-arid sites in the Columbia Plateau, we reiterate our concern about the prevailing concept of the "dry soil" zone above the water table as a zone which is never completely penetrated by rain water or snow melt and which therefore is a safe container for adsorbed radionuclides. We urge that AEC arrange for continuation and intensification of field investigations to determine the *amount* and *rate of* precipitation necessary to establish percolation to the water table in various types and thicknesses of soil and rock, both at Hanford and at NRTS. A study also should be made to determine the possible extent of upward movement of radionuclides to the root zone and thence to the surface along with water which reaches the surface and is transpired or evaporated.

The Committee would like to be able to offer some suggestions for research leading to better predictability of the results of unforeseen changes in the hydrology of a disposal reservoir, but the question is not an easy one to answer. Both man-made changes such as those caused by dams, irrigation projects and increased withdrawal rates, and natural changes resulting from catastrophic floods, slower processes such as erosion and channel cutting, or even long-term climatic changes, were considered. Continuing laboratory studies of reversible ion-exchange phenomena are desirable, but of course field studies eventually will be needed.

Probably the problem as a whole is a special case within the spectrum of "recharge" problems that are so poorly understood. Actually there is relatively little knowledge of *infiltration rates and mechanisms*, percolation rates, and other processes—especially through great thicknesses of unsaturated earth materials. The suggestion is offered that AEC support fundamental research on recharge mechanisms in the environments in which disposals must be conducted. This research would involve sophisticated analyses of precipitation, runoff, soil moisture, and water-level records as well as terrain studies involving the pedology and geology of selected sites. Artificial recharge floods and tracer studies might also be involved, despite the fairly large number of investigations that have already been carried out.

4. Deep-well disposals.—The Committee doubts that the deep subsurface geologic and hydrologic *conditions beneath* the lava series at Hanford and NRTS are suitable for disposal of waste liquids. In April 1961 the Subcommittee on Atomic Waste Disposal of the American Association of Petroleum Geologists, in selecting provinces for study of deep-well disposal potentials, rejected all parts of the Basin-and-Range province and other regions having similar tectonic complexities, because of the likelihood of structural features that would promote

the escape of radionuclides. In more stable tectonic provinces, however, the method holds much promise for the disposal of waste liquids.

Recently aroused public concern about the seismic activity that is said to *result from deep-well injections of toxic liquids into crystalline rocks of the* Precambrian basement near Denver, Colorado, points up the need for understanding all facets of subsurface technology in selecting disposal sites and reservoirs. Because the hydrology of basement rocks has not been studied to any appreciable extent, the Committee's recommendations are restricted to deep-well disposals in permeable sedimentary strata whose fluid mechanics are largely those of granular rather than fractured aquifers.

In view of the anticipated need for suitable new plant sites as the nuclear industry expands, development of a program of exploratory drilling in geologically acceptable basins should not be long delayed. Guidance and aid can be obtained by employing on a permanent basis geologists and engineers who have had experience in petroleum exploration. As an alternate procedure, a contract with a major oil company which has broad experience as well as complete research and operational facilities for this kind of exploration, would be useful. As for costs, a large amount of footage can be drilled and tested with an investment of $1,000,000 to $5,000,000, the exact amount depending on such factors as remoteness, terrain, and the character of the subsurface lithology. Costs can be reduced by joining forces with the petroleum industry if its borings are in areas of interest to AEC. Exploratory holes in locations which prove to be suitable for waste disposal can readily be converted to operational input wells or monitor holes. In the long run, drilling and testing costs will be a small part of the overall disposal program.

As for safety, the surface equipment is no more susceptible to accidents than that which is being used for hydraulic fracturing and grout injections at ORNL. Once *underground in properly selected reservoir formations, the radioactivity* is permanently shielded by thousands of feet of rock, a safer and more certain disposal than some other methods that have been considered. Provided sufficient time has elapsed in temporary surface storage to permit cooling to a satisfactory degree, even high-level waste liquids of suitable quality can be injected safely into deep permeable strata, although at present the technique is being considered only for low-level liquids.

5. Bedrock storage at Savannah River Plant.—Since the inception of the research project for bedrock storage at SRP this Committee has favored continuing investigation to determine the feasibility of the proposal, hoping that it would *lead to a more acceptable method than storage in surface tanks. At the same* time, however, the Committee has retained doubts that a permanently leakproof chamber can be constructed in the Precambrian bedrock at the plant site and that the proposed disposal method would provide complete protection against contamination of fresh-water aquifers. It was realized that the answers to these questions could come only from detailed subsurface testing *in situ*.

A large volume of hydrologic data has now been collected and is being studied, but the Committee *is unable to agree that the integrity of the system* has been proven. Still more study of the data, and further testing would be required for complete understanding of the hydrologic environments in the subsurface. The period of field research involving studies with observation wells apparently would be much more prolonged than was first envisioned, inasmuch as a totally new branch of the science of subsurface hydrodynamics is being explored. It may will require several more years of experimentation to prove that leakage *from the* bedrock into the overlying Tuscaloosa aquifers will not occur at some place within the basement area that would be invaded by escaped contaminants.

In its own appraisal of safety factors, SRP cites three separate "barriers" *(delay factors)* that, in its opinion, will protect *man's environment from invasion* by high-level wastes if they are emplaced in a storage chamber in the bedrock:

(1) The time required for hazardous radionuclides to migrate from the storage chamber through "sound" (unfractured or slightly fractured) rock to a major fracture.

(2) The time required for the radionuclides to migrate through a network of major fractures to a discharge point at the Savannah River.

(3) In the event that radionuclides should escape into the overlying Tuscaloosa Formation, the efficiency of natural ion-exchange agents in the aquifer and the additional time required for migration through the aquifer to the river.

The appraisal starts with the premise that the nearest point of risk is the Savannah River, assuming the presence of effectively impermeable layers be-

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tween the Tuscaloosa aquifers and the surface. On the contrary, it is the Committee's opinion that the fresh-water reserves in the combined Tuscaloosa aquifers are the first points of risk, and important ones, because they are *certainly going to be* drawn on increasingly for human consumption in approaching decades. In the event of future heavy withdrawals from the Tuscaloosa aquifers, the *increasing differences* between the head in those aquifers and that in the basement system would tend to increase any flow from the basement into the Tuscaloosa which might be too small today to be discerned. *The thought* of using the Tuscaloosa aquifer as an ion-exchange basin, in view of the prospect that future alterations in the hydrologic system may reverse the exchange *equilibria* and thus release sorbed nuclides, is hardly acceptable. Contamination of Tuscaloosa water in excess of five billionths of one part per million, which is the accepted tolerance for strontium ^{90}, would create an intolerable situation.

There is doubt and uncertainty concerning the mathematical assumptions in computing fluid movements through *fractured crystalline rock. Little is known* about hydrologic systems in this kind of environment, and this inadequacy in itself argues for contamination of the investigation *for a long time. All that is known* today about the hydrology of the Precambrian basement system beneath the Separations Plants is based on data from seven boreholes in a triangular area *of* about one square mile between the two plants. The only other point of control nearer than Aiken, a town 20 miles north, is the deep hole near the Savannah River in the *west corner of the* SRP reservation, about seven miles west-southwest of the Separations Areas. When additional wells are drilled down-gradient (the direction in which *contaminants would travel*), revisions in flow estimates may be required, especially if a boring should penetrate a fault-gouge or breccia zone where flow rates may be one or more orders of *magnitude higher than that of* the highest previous estimate.

In view of the complex lithology of the basement rock and of the intensive *deformation* to which it has been subjected, the Committee doubts that all fractures at a prospective excavation site can be located in advance of digging a shaft. *For that matter*, the probability of escape of some of the radioactive wastes from the chamber seems to be generally accepted. The uncertainty of the prospects for locating all fractures in the *vicinity of the chamber, and measuring* their permeabilities, makes it difficult if not impossible to provide reliable forecasts of the travel time for migrating nuclides.

The major uncertainty in the entire feasibility study, however, is the integrity of the saprolite clay layer as a continuous impermeable barrier between the bedrock and the Tuscaloosa aquifers throughout the area of possible contamination of bedrock waters. Prerequisites to proof of the barrier's effectiveness include (1) hydrologic data *from* the entire area in the subsurface that might be invaded by hazardous concentrations of radioactivity, (2) correct interpretation of the data and valid computations of hydrologic parameters, and (3) elimination of the possibility of openings caused by old stream channels or by knobs or ridges in the buried Precambrian surface. Sonic (acoustic) logs which may *indicate* the suitability of high-frequency seismographic mapping to determine the extent of the clay layer would be desirable.

In summary, it can be seen that there is doubt that it will be possible to prove safety of the proposed bedrock-storage system for high-level liquid or soluble wastes. Acting on the views of the majority of the Committee members, and still recognizing the existence of many uncertainties, the Committee recommends that investigations toward bedrock storage at SRP be *discontinued*.

At the same time, the entire Committee urges against any thought of permanent storage or disposal of high-level wastes above or in any of the fresh-water aquifers at the SRP site. As the recommendation regarding bedrock disposal is based primarily on inadequacy of safeguards against contamination of the fresh waters, disposal above or directly into those aquifers is obviously even more to be avoided. Apparently the only safe disposal for high-level wastes would be an offsite disposal, presumably involving *solidification* before transportation.

D. Planning for the future

AEC's long-range plans for future waste-disposal procedures are being made carefully and cautiously, as befits an operation that involves increasing volumes of elusive, hazardous materials. Recognition of confining parameters in the environmental characteristics which determine waste-disposal potentials at existing plant sites provides *a clear and reliable* guide to some of the criteria by which the suitability of any proposed plant site must be judged.

Sites for the plants where major *waste-disposal operations* are now being conducted are inadequate in one way or another with respect to ground dis-

posals of radioactive wastes. Although such shortcomings are understandable in view of the limited knowledge of disposal problems and techniques a decade or more ago, the current development of plans to make more complete use of the environment for future, more voluminous, disposals at most of the sites is noted with considerable anxiety. In light of today's more advanced, *though still in*sufficient, understanding of the geological, chemical, and physical parameters at all of the sites, major efforts are still being devoted to on-site disposals of intermediate and low-level wastes and storage of high-level wastes, some of which methods display the character of expedients designed to make the best *use of poor locations. For the future, it is hoped that either off-site* disposals of calcined solids, a costly alternative involving transportation of dangerous materials, or reductions in the output of radionuclides as other processes or other plants are developed, will alleviate the situation.

In this connection, the Committee dissents from the working philosophy of some operators, although certainly not that of AEC, that safety and economy are factors of equal weight in radioactive-waste disposal, and that the relative desirabilities of disposal practices can be assessed on the basis of hazard times cost. Certainly the present problems of stream pollution by industrial and municipal wastes have arisen *from the use of this philosophy in the past*. The Committee remains convinced that economics is a criterion secondary to that of safety.

In considering the many restrictions that are necessarily imposed on the methods of radioactive-waste disposal, and the wide variety of geologic, hydrologic and other conditions at different sites, the Committee is especially aware of the fact that privately owned plants are handling hazardous wastes and that *in the future even more such private operations will be* initiated. In view of the need for tailoring the disposal methods to suit local conditions at each site, the Committee hopes that authority to regulate the disposals will continue to be vested in AEC, and that AEC constantly will exercise its responsibility to see that safe disposal practices always are followed. In site selections as well as in subsequent operations, it is hoped that AEC will make sure that its contractors and licensees take full advantage of modern knowledge in subsurface geology and hydrology, *reservoir engineering, and drilling and testing* technology. A secure future in safe disposal practices demands nothing less than the best know-how that American industry has developed.

XI. RÉSUMÉ OF RECOMMENDATIONS

Listed below are the recommendations that are included in the various discussions in the preceding pages. They are recapitulated here for quick perusal. For full explanations, references are made to the pages where they are discussed in detail.

A. Disposal in salt

1. Study of the effect of heat and nuclear radiation on shale interbeds is suggested. (VI–A–4)

B. Grout injections in shale

1. The methods used at ORNL should be re-evaluated periodically. (X–C–1)
2. Each site and operation must be evaluated individually. (VI–B–4, first paragraph)
3. Each prospective disposal *formation* must be fractured experimentally in order to determine pressure requirements. (VI–B–4, second paragraph)
4. Local conditions should be investigated and pilot tests conducted before a site and disposal formation are accepted. (VI–B–4, third paragraph)

C. Solidification of liquid waste

1. Surface storage of calcined wastes above or in fresh-water aquifers appears unwise; proposals for such handling should be examined *carefully* (VI–C, fourth paragraph; X–C–2, last paragraph).

D. Ion-exchange studies

1. Saturated sorbants must be considered, and disposed of, as intermediate-level waste (VI–D–1, second paragraph).
2. Cumulative concentrations of sorbed radionuclides in the ground need to be monitored carefully, as the distribution pattern *will not* be known accurately. Future alterations in the hydrologic system may desorb and move them. Precautions should be taken to avoid this risk (VI–D–1, third paragraph; –2; –6; X–C–2, third paragraph).

3. Inasmuch as ion-exchange capacities in the ground are limited at most of the current operating sites, these sites should be reserved for accidental releases rather than used for routine disposals (VI–D–2; –3; –4; X–C–2, first paragraph).

4. The use of clinoptilolite or other ion-exchange materials in storage basins is recommended in preference to reliance on the ion-exchange capacity of earth materials in the ground (VI–D–4).

5. Studies of the movement of radionuclides in places where pits have leaked or overflowed at ORNL are suggested, as an opportunity to make use of a ready-made field laboratory (X–C–2, second paragraph).

6. Operators are cautioned against overloading the safe capacity of the Columbia River by effluents from the Ringold Formation (VI–D–5, second paragraph).

E. Bedrock storage at Savannah River Plant

1. The majority opinion of the Committee is that investigations toward bedrock disposal at SRP should be discontinued (X–C–5, next to last paragraph).

2. High-level wastes should not be stored permanently or disposed of above or in fresh-water aquifers at SRP (X–C–5, last paragraph).

3. A minority opinion of the Committee is that work on bedrock disposals at SRP should be continued, as follows:

(*a*) It is necessary to develop computation methods for the hydrology of fractured aquifers, and then to re-analyze all bedrock data at SRP by the method, to replace the analyses that have been based on the hydrology of homogeneous granular aquifers. The data used in constructing the regional potentiometric map of the Precambrian basement complex likewise should be re-evaluated (VI–E–3–b; –c; –4).

(*b*) If justified by results of the re-analyses, new and more carefully controlled tests in existing observations wells should be conducted, using continuous recording instruments (VI–E–3–c; –4).

(*c*) If justified by the results of new tests in existing wells, additional observation wells should be drilled and tested at locations including, eventually, the areas down-gradient from proposed disposal sites (VI–E–4).

(*d*) Full use should be made of tritium-tracer testing in order to gain direct evidence of direction and rates of movement (VI–E–3–b, last paragraph; —4).

(*e*) Sonic (acoustic) logs should be obtained in all boreholes, from top to bottom, before casing is run. If velocity characteristics of the rock are found to be suitable, high-frequency seismographic mapping should be attempted in order to learn the thickness and distribution of the clay layer at the base of the sedimentary section (VI–E–2, second through fifth paragraphs; VI–E–4, last three paragraphs; X–C–5).

(*f*) In drilling any new boreholes, samples of the clay at the base of the sedimentary section should be collected and analyzed (VI–E–4).

(*g*) Visits to deep mines in crystalline rocks at various locations in the Piedmont province, where the Precambrian basement is at or near the surface, will provide first-hand information on the problems of water flow into and out of such excavations (X–C–4).

(*h*) Periodic reviews of considerations for accepting or rejecting the bedrock storage proposal will be required as new data and testing results are analyzed (X–C–4).

F. Deep-well disposal

1. Now is the time to develop a program of exploratory drilling and testing in geologically acceptable basin provinces, in order to determine the thickness, petrography (including clay characteristics), porosity, permeability, and pressure patterns in prospective deep disposal reservoirs (VI–F–3, first paragraph; —4; —5; X–C–4, last two paragraphs).

2. Costs of this program can be reduced substantially by participating in any contemporaneous exploratory drilling that may be under way by the oil industry in the area of interest (X–C–4, third paragraph).

3. A requirement for a suitable disposal site is a thick layer of soft rock between the disposal reservoir and any stratum which must be protected from invasion. In general, a depth of about 3000 feet is minimum for a deep permeable disposal reservoir (VI–F–1, second paragraph; VI–F–3, last two paragraphs).

4. It is essential to avoid disposal into any reservoir from which fluids later may be withdrawn as mineral resources; i.e., fluids such as fresh water, brines of great commercial value, and petroleum (VI–F–3).

5. It is important also to avoid conflict with competitive disposal operations in the same reservoir. Records of pressure data should be continuously and permanently maintained in order to ensure awareness of possible conflicts (VI–F–5, second paragraph).

6. In order to conserve reservoir space it is deemed advisable to concentrate waste liquids as much as possible before injection. Injected fluids should be chemically stable, bacteriologically sterile, and nonreactive with respect to reservoir rocks and fluids (VI–F–3; —4, first paragraph; —5, third paragraph).

7. The disposal process must not permit access of radioactivity to the vicinity of fresh-water aquifers by leakage either within or behind the casing, or through fractures in rock strata above the disposal reservoir. (VI–F–3, first paragraph). Precautions should include preliminary test injections of nonradioactive fluids, except for tracer amounts, then surveying the hole with fracture-detecting instruments, and monitoring the bottom-hole pressures during injections of wastes (VI–F–3, third from last paragraph).

8. Pilot injections at prospective plant sites are prerequisites to final site acceptance (VI–F–3, last paragraph; —4, first two paragraphs; —5, last paragraph).

G. Geophysical studies at National Reactor Testing Station

1. Accurate data regarding the physical characteristics of the rock column at any exploration site should be acquired by laboratory analyses, not only before but also continuously during geophysical exploration. Without adequate control, interpretations of log and map data are apt to be misleading. Interpretations of geophysical mapping should be supported by analyses of samples taken from deep exploration holes (VI–G–1, fifth paragraph; –3; –4).

2. The geophysical program should be continued until its usefulness has been exhausted (VI–G–4).

H. Ground disposal of gaseous wastes

1. The proposed methods for ground disposal of gaseous wastes are considered to be impractical (VI–H).

I. Deep exploratory drilling

1. A deep exploratory well at NRTS is highly desirable in order to obtain essential geologic and hydrologic data by coring, geophysical logging, and the collection and analysis of samples of rocks and fluids (VIII–A).

2. It is recommended that the 10,655-foot boring that was abandoned by an oil company in 1958 as an unsuccessful exploration hole in the Rattlesnake Hills, near HAPO, be re-entered for the purpose of obtaining needed deep data from analyses of samples, fluid measurements, and geophysical logs (VIII–B).

J. Disposal vaults in desert hills

1. Detailed geological investigations of topographic highs in semi-arid or desert regions, such as Rattlesnake Hills (HAPO) and Middle Butte (NRTS), should be conducted in order to determine the feasibility of using them for sites of storage or disposal facilities for solid wastes (VI–C, last paragraph; IX).

K. Hydrology

1. Field and laboratory studies of the rate of movement of fluids, sorption capacities, and leaching and migration of radionuclides in rocks and soils should be continued persistently (V, fourth paragraph; numerous other discussions).

2. Study of the movement of water in three dimensions in the saturated zone should be diligently pursued at all major sites of AEC (IV–C; VI–G–4; X–C–3, first paragraph).

3. The movement of water, both upward and downward, under varying conditions of wetting in the zone of aeration at NRTS and HAPO should be thoroughly studied, particularly with reference to questions about percolation of rain water and snow melt to the water table (VII–D, last paragraph; X–C–3, second paragraph).

4. It is suggested that AEC support fundamental research on recharge mechanisms (X–C–3, last paragraph).

L. General

1. There is need for a standard classification of radioactive wastes, one based not only on the concentration of activity but also on the longevity of the nuclides in various ratios (IV–A).

2. There is need for AEC to establish a discipline of waste disposal as a guide for private operators as well as AEC contractors (V, last paragraph).

3. Safe disposal procedures should be established now rather than at later stages of power development (V, fifth paragraph; X–C–2, first paragraph).

4. **Present sites appear to be unsuited for ground disposals of large amounts of long-lived radionuclides. Safer disposal to the subsurface can be developed in geologic basins in the stable interior of the continent where fresh-water aquifers are separated from potential waste-disposal reservoirs by thousands of feet of layered strata (IV–B–5, next to last paragraph; X–D, second paragraph).**

5. **In the general field of subsurface waste disposal, AEC and its principal contractors should exploit industrial technology in which subsurface investigation has reached high levels of sophistication. Closer liaison should be developed between waste-disposal technologists and major industrial research laboratories for technologic exchange in problems of mutual interest such as formation testing, ion-exchange, hydraulic fracturing, geophysical-log interpretation, and fluid injection in porous rocks (VI–B, last paragraph; VI–F–3, second paragraph; VI–F–4, second and third paragraphs; VIII; X–C–4, third paragraph).**

6. **No study of any of the phenomena involved in any means of waste disposal should be neglected because of lack of funds. Funds should be provided as necessary to insure a complete development of both practice and theory of safe waste disposal. We particularly call attention to the necessity for field studies (V, third paragraph; X–B; X–D, third paragraph).**

TABLE III

Stratigraphic table

MAJOR DIVISIONS	SYSTEMS	SERIES	FORMATION NAMES USED IN TEXT	APPROX. AGE (Million years)
Cenozoic	Quaternary	Recent		
		Pleistocene		1
	Tertiary	Pliocene	Ringold	10
		Miocene	Ellensburg; Columbia River and Snake River Lavas	
		Oligocene		
		Eocene		60
		Paleocene		
Mesozoic	Cretaceous	Upper	Tuscaloosa	
		Lower		125
	Jurassic			
	Triassic			180
Paleozoic	Permian	Ochoa		
		Guadalupe		
		Leonard	Wellington	
		Wolfcamp		
	Pennsylvanian	Virgil		
		Missouri	Chanute	
		Des Moines		
		Atoka		
		Morrow		
	Mississippian			255
	Devonian			
	Silurian			350
	Ordovician		Chickamauga	
	Cambrian		Conasauga	
			Rome Sandstone	510
Precambrian	Precambrian	Precambrian		

Glossary of Words and Terms Used in the Text

AAPG—American Association of Petroleum Geologists.

AEC—United States Atomic Energy Commission.

Aeolian—Eroded, produced, borne, or deposited by wind.

Alluvial—Pertaining to or composed of alluvium.

Alluvium—Detrital material deposited by running water.

Aquiclude—"A formation which, although porous and capable of absorbing water slowly, will not transmit it fast enough to furnish an appreciable supply for a well or spring." (Tolman)

Aquifer—A subsurface stratum (or group of strata) of sand, gravel or other unconsolidated permeable sediments, or of indurated but porous and permeable rock, containing water. Usage is usually restricted to beds carrying fresh water in enough quantity to be a source of supply. See "Salaquifer."

Basalt—A dark fine-grained or dense igneous rock of volcanic origin. Much of it is vesicular—that is, it contains small cavities. In fractured basalt the cavities add to the permeability.

Basement—That portion of the earth's crust consisting usually of crystalline rock, igneous or metamorphic, having complex structure and underlying generally unmetamorphosed, less strongly folded sedimentary rocks. In oilfield usage, the surface of the basement is the "floor" below which exploration for oil and gas is usually considered to be useless. The age of the basement rocks is Precambrian at most places, but younger rocks may be included in some regions.

Basin—(Subsurface geology)—A depression in a geological surface. It may or may not have a topographic expression. If caused by tectonic events which create a downwarp of the strata, it is called a *structural basin*. If it is a catchment area filled (or being filled) with sediments, it is referred to as a *sedimentary basin*. The hydrology within a basin is usually an integrated system which is controlled by the shape of the basin as well as by the character of the rocks in it. A sedimentary basin may be affected by post-depositional tectonism, in which case the integrity of the original system will have been altered or destroyed and eventually replaced by that of a new system.

Bedrock—(1) Solid or indurated rock underlying the surface veneer of soil and unconsolidated sediments. (2) In local usage, the basement.

Biosphere—That portion of the earth occupied by organisms (excluding bacteria which may live in subsurface waters at great depths).

Breccia—A rock composed of angular fragments of moderate or large size.

Calcine (verb)—To reduce to a powder, or to a friable state, by the action of heat; to heat so as to expel volatile matter from; to oxidize, as by the action of heat. (Webster's New International Dictionary: G. and C. Merriam Company.)

Cambrian—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

Clastic—Composed of transported fragments of pre-existing rocks.

Clay—A soft rock consisting of very small (less than 1/256-mm diameter) mineral fragments, particularly various clay minerals which are largely hydrous aluminum silicates. Other than the clay-mineral content and the very fine-grained texture, clay is characterized by its property of being plastic when wet.

Cretaceous—A division of geologic time and of a stratigraphic sequence (See "Stratigraphy")

Crystalline—In rock descriptions, composed of interlocking crystals of one or more minerals. Granite, gneiss, anhydrite are examples of crystalline rocks.

Curi—That quality of a radioactive nuclide disintegrating at the rate of 3.7×10^{10} atoms per second, which is approximately the rate of decay in one gram of natural radium.

Disposal—Putting materials away in a manner or place that renders them practically irretrievable. (See "Storage")

Eolian—See "Aeolian."

Evapotranspiration—Loss of water from the ground by a combination of evaporation and plant transpiration.

Exposure—In geology, a rock surface that is exposed to view, not covered by soil or water.

Fault—A fracture in the earth's crust accompanied by a displacement of one side of the fracture with respect to the other and in a direction parallel to the plane of fracture.

Fluviatile—Pertaining to streams or rivers.

Foliated—Separation into thin plates, sheets, or slabs, as in slate.

Formation—In stratigraphic geology, a sedimentary bed (layer) or series of beds sufficiently distinctive to be regarded as a unit; a subdivision of "group" or "series."

Geohydrology—The science treating of water in the earth. (See "hydrology")

Geology—The science which treats of the earth, in all its aspects.

Geophysics—The science of the earth with respect to its structure, composition, and development.

Gouge—Finely abraded material occurring between the walls of a fault as a result of the grinding movement.

Ground water (Underground water)—Subsurface water occupying the zone of saturation (q.v.). Phreatic water.

Grout—Thin mortar, fluid enough to be pumped, used for filling joints, fractures, etc.

HAPO—Hanford Atomic Products Operation, southeastern Washington.

High-level waste—See chapter IV–A.

Hydraulic fracturing—Creation of fractures by the application of pressure through a liquid.

Hydraulic gradient—As applied to an aquifer, the rate of drop of the total head in a horizontal direction in which this rate of decline is a maximum.

Hydrogeology—The part of geology that is concerned with the interaction of water and the geologic framework.

Hydrology—The science that treats of the water of the earth. Some scientists use the term with reference specifically to underground water, as distinguished from *hydrography*, which is applied to surface water.

Igneous rock—Rock which was formed by cooling and solidification from a molten state. The individual mineral crystals may be microscopic in size, as in basalt, or large, as in coarse-grained granite. The rock types (granite, basalt, etc.) are determined by the texture and by the proportions of different minerals of which the rock is composed.

Intermediate-level waste—See chapter IV–A.

Joint—A fracture in rock, smaller than a fissure or fault and not accompanied by dislocation.

Kaolinite—A common clay mineral; a hydrous silicate of aluminum, derived from the decomposition of aluminous minerals, especially feldspar.

Lacustrine—Pertaining to lakes.

Lava—Fluid rock such as that which issues from a volcano or fissure in the earth's crust; also, the same material solidified by cooling.

Lithology—The study of rocks based on megascopic examination of samples; also, loosely, the composition and texture of rock.

Low-level waste—See chapter IV–A.

Mesozoic—A major division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

Metamorphic rock—Rock which was formed in the solid state by the alteration (metamorphosis) or pre-existing rock under conditions of elevated temperatures and pressures or changed chemical environment.

Microcurie—One-millionth of a curie (q.v.).

Millicurie—One-thousandth of a curie (q.v.).

Mineral—A homogeneous naturally occurring, usually inorganic and crystalline substance.

MPC—Maximum Permissible Concentration of radioactivity, as defined by the National Council on Radiation Protection and Measurements. The term has been replaced by "Radioactivity Concentration Guide" (RCG, q.v.).

NRTS—National Reactor Testing Station, near Idaho Falls, Idaho.

Ordovician—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

ORNL—Oak Ridge National Laboratory, Tennessee.

Outcrop—That part of a stratigraphic unit which lies at the surface. It may be covered by soil or water; if not, it is also referred to as an *exposure*.

Paleozoic—A major division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

Pennsylvanian—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

Permian—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")

Petrography—The description and systematic classification of rocks; it usually involves chemical analysis or microscopic examination of thin sections.

Playa—The shallow central basin of a desert plain, in which water gathers after a rain and is evaporated.

Potentiometric gradient—See *Hydraulic gradient*.

Precambrian—All rocks and time older than Cambrian. See "Stratigraphy."

Pyroclastic—A general term applied to detrital volcanic materials, such as volcanic ash, that have been explosively or aerially ejected from a volcanic vent; also a general term for the class of rocks made up of these materials.

Quaternary—A division of geologic time and of a stratigraphic sequence (See "Stratigraphy")

Radiohydrology—The science treating of surface or ground water which contains radioactive elements.

RCG—Radioactivity Concentration Guide, as defined by the Federal Radiation Council.

Recent (capitalized)—The interval of geologic time since the end of the Pleistocene epoch, and the rocks or geologic events associated with it. (See "Stratigraphy")

Reservoir—A natural underground container of fluids. In oilfield usage, a deep statum of porous and permeable rock such as sandstone or limestone.

Rock—Strictly, any naturally formed aggregate or mass of mineral matter, whether or not coherent, constituting an essential and appreciable part of the earth's crust. Ordinarily, any solid or coherent and relatively hard, naturally formed mass of mineral matter.

Salaquifer—An aquifer (q.v.) carrying saline water, commonly deeper than fresh-water aquifers.

Sand—Loose sediment (q.v.) composed of mineral or rocks fragments ranging in diameter between 1/16 and 2 mm.

Sandface—The vertical cut surface of a sandstone or other permeable rock layer in a borehole.

Sandstone—An indurated sedimentary rock (q.v.) formed by natural cementation of sand.

Saprolite—Disintegrated rock, more or less decomposed, which lies in its original place.

Sediment—Material in suspension or recently deposited from the waters of streams, lakes or seas, and in a more general sense deposits of wind and ice. Sediment typically consists of loose and unconsolidated fragments of rock. The specific type is determined by the size of the grains or fragments (clay, silt, sand, gravel). Purists consider a sediment to be a type of rock.

Sedimentary rock—Indurated sediment, such as claystone, siltstone, sandstone, conglomerate, limestone. A characteristic feature of most sedimentary rocks is a layered composition, each layer being a bed or stratum.

Series—A subdivision of the stratigraphic sequence in a rock column. (See "Stratigraphy")

Shale—Laminated or fissile claystone or siltstone.

Soil—(1) Agriculture: The loose surface material of the earth in which plants grow. (2) Geology: The surface zone or veneer of loose materials consisting of particles derived by decay or disintegration of rock and usually mixed with vegetal and animal products. (3) Waste-disposal usage: Sediments and sedimentary rocks; apparently often used to include all rock above the basement.

SRP—Savannah River Plant, South Carolina.

Storage—Putting materials away with the intent to retrieve them, or in such manner that retrieval is feasible. (See "Disposal")

Stratigraphy—Study of the strata and their interrelationships in a sequence of layered rocks. Proper names apply to both time units and rock-age units. Table III (q.v.) shows major and lesser subdivisions, including all formation names that are used in the text of this report. Not all divisions are present at all locations; for example, Paleozoic strata are missing at SRP, where Mesozoic strata lie directly on the Precambrian basement.

Structure—(tectonic)—The attitude and relative positions of rock masses consequent upon deformative processes such as folding, faulting, and igneous intrusion.

Subsurface geology—The study of rocks and their fluid contents beneath land or sea-floor surfaces by means of drilling and geophysical surveys.

Synclinal—Formed by strata dipping toward a common line or plane, making a downward or trough-shaped flexure in folded rocks.

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Tectonic—Referring to rock structure and its external forms resulting from the deformation of the earth's crust. As applied to earthquakes, it is used to describe shocks that are not due to volcanism or to collapse of caverns or to landslides.
Tertiary—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")
Transmissibility—Capability for permitting a flow of contained fluids. Statis-
tically, permeability multiplied by thickness.
Triassic—A division of geologic time and of a stratigraphic sequence. (See "Stratigraphy")
USGS—United States Geological Survey.
Vadose—Referring to water in the zone of aeration, above the water table.
Water table—The surface whose elevation at each point is that of the level at which water will stand in an open hole whose total depth is just below that surface.
Zone of aeration—The part of the earth's crust lying above the zone of saturation. Any water zone of aeration either is held in interstices by molecular attraction or capillary forces, or is moving downward toward the zone of saturation, and is known as *vadose* water.
Zone of saturation—The part of the earth's curst in which permeable rocks are saturated with water under hydrostatic pressure (in excess of the atmospheric pressure). The water filling the interstices is called *ground water*.

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Comments on the Background of the May 1966 Report of the NAS Committee on Geologic Aspects of Radioactive Waste Disposal

In May 1966, the Committee on Geologic Aspects of Radioactive Waste Dis-
posal of the Earth Sciences Division, National Academy of Sciences-National
Research Council, forwarded a report to the Division of Reactor Development
and Technology, U.S. Atomic Energy Commission. The scope of the work of
this Committee is shown as follows in the first paragraph of the report's
introduction:

"The work of the Committee on Geologic Aspects of Waste Disposal of the
National Academy of Sciences-National Research Council (NAS-NRC) is sup-
ported by the Division of Reactor Development and Technology of the United
States Atomic Energy Commission (AEC), whom the Committee serves as
adviser. The Committee's responsibility to that Division is to observe and study
critically the research and development activities of the Division with respect
to radioactive waste disposals in the ground, and to provide counsel regarding
the safety of the Division's current and proposed operations insofar as they
are affected by geologic considerations."

Various committees, advisory to AEC, on radioactive waste disposal R&D
programs have existed within the NAS-NRC Earth Sciences Division since
1955, and with some degree of continuity of membership. Although various
statements of such committees' functions were used over the entire period,
their basic purpose was always to advise on the research and development
related to radioactive waste management.

The AEC has considered the comments and suggestions of the Committee
on Geologic Aspects of Waste Disposal regarding AEC's research and develop-
ment program on radioactive waste management as a useful input to the con-
tinued planning of that program. General development programs profiting
from the Committee's encouragement and suggestions have included those
for hydraulic fracturing for intermediate level radioactive wastes, now reduced
to standard practice at Oak Ridge; high temperature solidification of high
level wastes now well advanced in an engineering prototype facility at Han-
ford; and salt mine burial of high-level wastes, which has received an extended
engineering test in the Lyons, Kansas mine. During the site visits leading to
the May 1966 report, a number of specific Committee suggestions were received
and put into effect, such as the measurement of vertical movement in wells
at the National Reactor Testing Station and the ideas for a more sophisticated
analysis of pumping test data at the Savannah River Project.

The Committee was not unanimous in all of its recommendations nor has the
AEC adopted them all. For example, in the May 1966 report a majority of the
Committee recommended discontinuance of development work on the concept of
disposal of high level wastes in a chamber to be mined in the solid bedrock which
lies below the Savannah River site. A minority opinion was that such investiga-
tions should be continued and technical guidance for such further effort was pro-
vided. AEC still considers that this concept is worth pursuing and is proceeding
with a program directed toward determining its feasibility.

Scope of Committee Responsibility

Despite the fact that the scope of Committee responsibility was limited to
advising the Division of Reactor Development on its research and development
program, and despite the fact that the Committee received relatively little infor-
mation concerning AEC operations, the Committee in 1960 developed conclusions
and recommendations regarding AEC's waste management operational activities.
This was the case even though certain important aspects of the Production
Division's waste management operations were classified and the Committee was
not willing to receive classified information.

In summary, these views were that no existing AEC installation which generates either high level or intermediate-level wastes appears to have a satisfactory geological location for the safe local disposal of such wate products; neither does any of the present waste-disposal practices satisfy the Committee's criterion for safe disposal of such wastes.

In 1960 the Committee made three recommendations: (1) that action be taken for the establishment of waste-disposal facilities at suitable geological sites where the accumulated wastes of the existing installations can be processed and safely disposed of; (2) that approved plans for the safe disposal of radioactive wastes be made a prerequisite for the approval of the site of any future installations by the AEC or under its jurisdiction; and (3) that the Commission consider concentrating its chemical processing activities at a minimum number of sites located at satisfactory places for the disposal of radioactive wastes.

These recommendations were made after the Committee had visited the AEC sites at Oak Ridge and Savannah River, but before it visited either Hanford or NRTS. Following its visits to the latter two sites, the Committee informed AEC that neither the Hanford plant nor the Idaho Falls plant was creating a hazard. However, the Committee commented that processing plants at sites selected because of their suitability for effective and complete safe waste disposal practices would be much preferred to chemical processing work at Idaho Falls or Hanford.

To understand the implications of the foregoing conclusions and recommendations, additional background information is necessary. AEC had already constructed major production reactor and fuel reprocessing facilities at Hanford, Washington, and Savannah River, S.C., and additional fuel reprocessing facilities had been built at NRTS in Idaho. Extensive radioactive waste management operations had been and were being conducted at each of the above installations, as well as in developmental facilities at Oak Ridge, Tennessee.

To comply with the Committee's recommendations, AEC would have had to abandon fuel reprocessing and radioactive waste management facilities and activities at each of the above sites. It would have had to acquire an extensive new site or sites, presumably located over either salt beds or deep synclinal basins, since such locations appeared most attractive to the Committee for disposal of waste. It would have had to construct new fuel processing facilities and waste management facilities at the new site and move existing radioactive wastes from existing sites to the new site for disposal. Such an undertaking would have involved the expenditure of billions of dollars.

In considering the Committee's recommendations the Commission pointed out that high level radioactive wastes were not being "disposed of" at any of AEC's sites, but merely stored pending development of satisfactory disposal methods. The Commission could see no necessity for going to the enormous expense of relocating existing fuel element processing facilities as implied in the Committee's recommendations.

The Commission also felt it necessary to clarify the Committee's postulate that "no system of waste disposal can be considered *safe* in which the wastes are not completely isolated from all living things for the period during which they are dangerous," pointing out that if this were intended to mean "that *zero* radioactivity should be allowed to reach man's environment" it would raise "fundamental questions including those of a biological and medical nature that are very broad in scope." Such a *zero* radioactivity requirement would not be consistent with recommendations of groups which are responsible for advising on radioactive exposure standards such as the Federal Radiation Council.

Most of AEC's criticisms of the May 1966 report relate to the inclusion and amplification of the Committee's 1960 conclusions and recommendations about AEC sites and its radioactive management operations. These were included despite advance attempts to reach an understanding (i.e. in 1965) that the Committee was to review the R&D program on radioactive waste management for which the Division of Reactor Development and Technology was responsible. In preparing the May 1966 report, the Committee made brief visits to a number of AEC sites and facilities, including operations sites where brief presentations concerning operational activities were made by Division of Production staff or by associated field office and contractor staff. The AEC considered these visits and briefings on its waste management operations to be incidental to the primary research and development program review mission of the Committee, but of possible value in providing background information related to experience in disposal of radioactively-contaminated wastes.

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As stated earlier, the Committee's comments and recommendations regarding RDT's radioactive waste management R&D program have been useful and have received careful consideration by AEC in planning its program. The May 1966 report has been utilized internally by AEC for this purpose but has not been published. AEC's concerns over the report were not resolved before the decision to dissolve the Committee and replace it with one having broader spectrum of scientific disciplines.

The present Academy Committee on Radioactive Waste Management (CRWM) has the following charter: ". . . to review and advise the AEC concering long-range radioactive waste management plans and programs for an expanding nuclear energy industry. This primary task would include the general assessment of the adequacy of the present and projected technology in relation to meeting long-range health, safety, and other environmental requirements, and the identification of new research needs." During the past year, the CRWM has visited Hanford, Oak Ridge, Savannah River, and NRTS. The CRWM has been briefed both on radioactive waste management R&D and operational practices at those sites. In view of the request for release of the May 1966 report, the Commission has requested the CRWM to report its comments regarding sites which it visited and the operational policies and practices which it has observed as well as the R&D programs. The May 1966 report has been available to the CRWM since it was organized.

Radioactive Waste Management: An Interim Report of the Committee on Radioactive Waste Management

(National Academy of Sciences, a National Research Council, February 17, 1970)

In 1968 the Committee on Radioactive Waste Management (CRWM) was formed by the National Academy of Sciences (NAS) at the request of the Atomic Energy Commission (AEC). The present membership consists of Professor Clark Goodman, Chairman; Mr. W. Kenneth Davis; Professor Robley D. Evans; Dr. John C. Frye; Professor Jack E. McKee; Mr. Herbert M. Parker; Professor John H. Rust; Dr. F. H. Spedding; Dr. Clarke Williams; Mr. Hood Worthington; Mr. John A. Erlewine, AEC Liaison; and Dr. Cyrus Klingsberg, Technical Secretary, NAS–NRC.

The general scope of the Committee's task is as follows:

Advise the AEC concerning long-range radioactive waste management plans and programs for an expanding nuclear energy industry. This primary task of the Committee will include the general assessment of the adequacy of the present and projected technology in relation to meeting long-range health, safety, and other environmental requirements, and the identification of new research needs.

Undertake specific studies on the initiative of either the Commission or the Committee following disagreement on the objectives of such studies.

Establish ad hoc panels, where necessary, to develop specific areas essential to the Committee's work.

One of the initial tasks will be to compile a comprehensive listing of various topics within the scope of the Committee's charter that could be considered by the NAS and the AEC as additional tasks for study by the Committee.

The Committee will not be requested to provide advice on specific cases that normally come within the purview of the AEC Advisory Committee on Reactor Safeguards.

The general scope of the Committee's task may be modified or extended as needed by mutual consent of the Atomic Energy Commission and the National Academy of Sciences.

The Committee on Radioactive Waste Management spent its first year visiting the major AEC sites to survey the present and anticipated practices involved in radioactive waste management. The Committee has been particularly attentive to the problems associated with the permanent storage of high-level radioactive wastes from nuclear fuel reprocessing plants.

Following its organizational meeting in Washington, D.C., on August 26, 1968, the CRWM held its first on-site visit at Oak Ridge National Laboratory (ORNL) on October 17–18, 1968. The pattern established here was followed at all future site visits. After formal presentations, which covered all aspects of that site's waste management programs, both operational and R&D, the Committee made a tour and saw firsthand the practices described in the presentations.

The topics covered at Oak Ridge included the following:

1. Low-, intermediate-, and high-level waste management,
2. Storage of high-level solidified waste in salt formations,
3. Noble gas removal,
4. Waste management in nuclear power stations,
5. Economic evaluations of fuel reprocessing waste management,
6. Siting fuel processing plants and waste management facilities,
7. Management of solid, gaseous, and liquid wastes,
8. Hydraulic fracturing, including rock mechanic aspects,
9. Environmental monitoring and radioecological research relevant to radioactive waste releases, and
10. A survey of the geohydrological characteristics of the ORNL site.

For the site tour, four ORNL locations were visited. The first was the central control room for the gas and liquid monitoring equipment where both monitoring and recording are continuous. The second building contained evaporator and

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waste condenser controls that are responsible for monitoring 99 percent of all ORNL waste. At the third location the Committee visited a modified water treatment plant, which used lime, soda ash, and clay to decontaminate the radioactive liquids. At the final stop, the hydrofracturing station was visited. It was not in operation at the time, since there are only two injections a year, each lasting about one day.

The third meeting of the Committee was held at the Savannah River Plant (SRP) on January 20–21, 1969.

Representatives of Du Pont (which is responsible for the overall management of the SRP) reported that Du Pont, in addition to cooperating with local boards of health and the U.S. Public Health Service, has supported an independent study by the Academy of Natural Sciences of Philadelphia on the biological condition of the river before the plant went into operation. It still conducts periodic surveys, and the results of these indicate that the operation of the SRP has had very little effect on the biological condition of the Savannah River.

Additional formal presentations included such topics as:

1. Long-term storage of highly active wastes in bedrock caverns,
2. Design and construction criteria for facilities to handle highly active liquid waste,
3. The handling and storage of separations process wastes,
4. Disposal of radioactively contaminated solids,
5. Disposal of contaminated water from the reactor areas,
6. Control guides and experience related to radioactivity release to the environs, and
7. Geology, hydrology, and geography of the site.

For its plant tour, the Committee observed a power boat collecting water samples from the Savannah River and a continuous floating sampler. An empty emergency holding basin (for reactor cooling water) of 50-million-gallon capacity was visited. The chemical processing plant was seen from the road but was not entered. The Committee saw completely buried, high-level storage tanks of 85-foot diameter and, nearby, four tanks, each of 1¼ million-gallon capacity, were observed under construction. The Committee entered a small monitoring building filled with waste-tank cooling coils in which the decay heat could be felt in some of the pipes. A drop in temperature observed at this location would allow the point of a leak in a tank's cooling coils to be located. A newly prepared burial ground was observed where containers of contaminated solids and empty contaminated water tanks from a Greenland airplane crash were being buried.

Prior to the fourth meeting of the Committee, a task force consisting of Goodman, Rust, and Klingsberg visited the Dresden Nuclear Power Station, Morris, Illinois, on April 16, 1969. Between 2 and 3 thousand cubic feet of solid low-level wastes will be handled there annually. The Sheffield Burial Site is approximately 50 miles to the west; it is managed by the Nuclear Engineering Corporation on ground that is owned by the state of Illinois.

On a site adjacent to the Dresden plant a fuel reprocessing plant is being built by General Electric (G.E.). This 300-ton-per-year plant is known as the Midwest Fuel Recovery Plant (MFRP).

In addition, Commonwealth Edison is constructing two additional large boiling water nuclear power plants, known as Dresden No. 2 and Dresden No. 3, which will be in operation in the very near future. The question of what level of stack discharge of radioactive gases will be permitted from each of the three Dresden plants plus the G.E. reprocessing plant is now being considered by the AEC Licensing and Regulations Division.

For its fourth meeting, the Committee visited the National Reactor Testing Station (NRTS), Idaho Falls, Idaho, April 17–18, 1969. The formal presentations included such topics as—

1. Radioactive waste management policy at NRTS,
2. Waste generation and management at test reactors, at the chemical processing plant, and at the Experimental Breeder Reactor II (EBR–II) project,
3. Solid waste disposal,
4. Calcination of liquid waste,
5. Management of radioactive waste proposed for the loss of fluid test (LOFT),
6. Radionuclide distribution in regional groundwater and in the regolith as a result of liquid waste disposal,

7. Environmental behavior of gaseous waste disposal in the atmosphere including prediction of trajectories and dispersion, and,

8. Geology and hydrology of the NRTS site.

For its plant tour, the Committee visited the Fuel Cycle Operations of EBR-II. A demonstration was held of a dump of a "cold" can into a burial hole at the EBR-II burial grounds. The Idaho Chemical Processing Plant was visited. This is a canyon building with a 240-foot operating corridor. The plant was not in operation at the time. Finally, the Committee visited the calciner, which employs a fluidized bed at 400°C.

The fifth meeting of the Committee was held at the Hanford Plant, Richland, Washington, June 23–24, 1969. The lectures included—

1. Hanford waste management policy covering the nature, present handling, and storage of chemical processing wastes and future options in long-term waste storage,
2. Handling of contaminated gaseous, solid, and liquid effluents in chemical processing plants,
3. Fuels and reactor waste disposal practices,
4. Waste solidification demonstration program (phosphate glass, spray solidification, pot calcination),
5. Movements of radionuclides through the local environment, and
6. General site description.

For the plant tour the Committee visited B Plant and saw a demonstration that is typical of a remotely operated and remotely maintained plant. The Committee visited a display of off-site shipping containers: a HAPO-II shipping container and its protective buffer container, small tunafish-type cans, birdcage containers, and 5-inch diameter plastic bottles. The Purex separation plant was visited next, and the Committee walked through the pipe and operating gallery. Two 1-million-gallon tanks of the SRP type were observed under construction. An exploratory well, 3100 feet in depth (which will be extended to 7500 feet when completed), was observed. At the Chemical and Materials Engineering Laboratory the Committee visited the fission product cask handling area. A 30-ton loading cask was observed through a shielded window. In addition, a 350-gallon on-site cask was observed close up.

Following these plant visits the Committee met with the AEC Commissioners in Germantown, Maryland, on October 9, 1969, in order to inform the AEC of the progress to date.

The seventh meeting of the Committee was held at the Atomic Industrial Forum, New York, on September 12, 1969. An ad hoc group of industrial representatives presented their points of view with respect to the problems of radioactive waste management.

The purpose of the tour of the major AEC installations was to provide an appropriate orientation for the Committee so that it would be brought up-to-date as to the current research and development programs under way at these AEC sites.

The Committee noted the extensiveness and care in waste management at each site visited. The Committee is gratified by the quality and scope of the R&D program sponsored by the AEC in radioactive waste management.

An alternative to perpetual tank storage of liquids is the conversion of the liquid solutions to thermally and radiolytically stable solids of low solubility for burial in suitable underground sites or for storage in excavations. Under the AEC program four solidification techniques have been examined in detail:

1. The fluidized bed calciner developed at the Idaho Chemical Processing Plant;
2. The phosphate glass process developed at Brookhaven and being tested at Hanford;
3. The spray solidification process developed and tested at Hanford; and
4. The pot calcination process developed at ORNL and being tested at Hanford.

The last three have been under full-scale tests at Hanford since November 1966 using fission products having thermal and radioactivity levels comparable to spent fuel from commercial power plants. These tests are scheduled for completion in 1970.

During the coming year, the CRWM expects to take into consideration:

1. Transportation, especially of high-level waste (relative safety of st age versus shipping),
2. Management of low-level wastes,
3. Review of cost evaluations of radioactive waste management, and
4. Analysis of AEC policy on high-level waste management, and
5. Exchange of information between the United States and ot countries.

In all of these studies, an attempt will be made to maintain a balanced vi point between radioactive wastes and all other types that could endanger environment.